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RESEARCHES IN OBSTETRICS

34 C

RESEARCHES

IN

OBSTETRICS

BY

J. MATTHEWS DUNCAN

A.M. M.D. L.R.C.S.E.

Lecturer on Midwifery in Surgeons' Hall Medical School, Physician for and Clinical Lecturer on
Diseases of Women in the Royal Infirmary,
Fellow and Member of Council of the Royal College of Physicians,
Fellow and Member of Council of the Royal Society;
Honorary Fellow of the Obstetrical Society of London,
of the Hunterian Medical Society, of the Imperial Royal Society of Physicians of Vienna,
of the North of England Obstetrical Society,
etc. etc.

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DEDICATED

TO THE

OBSTETRICAL SOCIETY OF LONDON

BY ONE OF ITS

HONORARY FELLOWS

PREFACE.

THE Essays composing this Volume are chiefly gathered from my writings in various medical and scientific periodicals. While all such have been revised, some of them have been so altered and added to as scarcely to be with justice called republications. A few are now for the first time in print.

The contents of this book owe their origin chiefly to studies undertaken with a view to make my public teaching of Midwifery more useful and interesting. The pursuit of these studies has been carried on in the midst of the exacting duties and anxious cares of medical practice. Were it not so, this book would have been made far more worthy than it is of being presented to my professional brethren. While, on this ground, I ask their indulgence to me as an author, I am glad to say that, with one exception, nothing has given me so much pleasure as the pursuit of obstetrical science, even when it was felt to be somewhat laborious.

30 CHARLOTTE SQUARE, EDINBURGH,
January 1868.

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PART I.

THE STATICS OF PREGNANCY.

RESEARCHES

IN

OBSTETRICAL SCIENCE.

PART I.

THE STATICS OF PREGNANCY.

THERE is no object more worthy of the attention of obstetricians than the elucidation of the anatomy and physiology of pregnancy.

The following researches have been made with this view.

Before commencing, I must guard the reader from supposing that in defining angles, etc., I wish to imply the real existence of mathematical exactness in the subject. When I use numbers in this way, it is merely to express a near, perhaps the nearest, approximation to the reality. And I resort to them in order to facilitate the application to the anatomical circumstances so expressed of mechanical principles, which can be illustrated only by mathematically exact statements.

CHAPTER I.

THE POSITION OF THE UTERUS.

In the healthy unimpregnated female, the uterus, an organ of about two and a half inches in length, generally lies with its fundus near the centre of the imaginary plane of the brim of the pelvis, and having its long axis in a direction corresponding to the axis of the brim, that is, an imaginary line drawn at right angles to the plane of the brim from its centre. (See *a b*, Fig. 1.) This is only a rough statement, and in the meantime it is sufficient to refer to the second plate of the work of Kohlrausch, *Zur Anatomie und Physiologie der Beckenorgane,* for the most accurate exhibition we possess of the position of the uterus.

The unimpregnated uterus, weighing about an ounce, is so delicately and unstably poised in its position, that the common description of it as *floating* there, is an extremely apt and happy use of that term. In accordance with this, we find that the first change produced by the increase of its weight and bulk in pregnancy, is that it sinks in the pelvis. Gradually increasing in size to dimensions greater than those of this bony ring, it rises out of that cavity, and is described as coming to rest upon its brim. But the uterus cannot, without risk of misapprehension, be described as resting upon the brim of the pelvis. These words imply that in pregnancy, the mass and weight of the uterus repose upon the alternately soft and muscular, or harder and bony, walls of the brim in the living female.

Although, as a general rule, the uterus remains more or less completely within the pelvic excavation for the first three months of pregnancy, it does not invariably do so. In special conditions of the retentive power of the abdomen, it rises into the abdominal cavity, even in the earliest months, and floats high without in any sense resting on the pelvis.

It must be remembered that the brim of the pelvis is in the erect posture, the attitude understood in all such discussions, much nearer vertical than horizontal, being inclined to the horizon at an angle (see Fig. 1) of 60°, a position in which it is evident it can give no support in the ordinary acceptation of that term. The brim of the bony pelvis has an inclination of 60° to the horizon, according to the researches of Nægele on the female. This angle is increased by the psoæ and iliaci muscles, which cover the sides of the brim, being thicker superiorly than inferiorly. If we turn to the body to be supported, we shall find its circumstances totally inconsistent with this description. The long axis of the uterus is inclined to the horizon at about the same angle as the axis of the brim—that is, an angle of about 30° (see Fig. 1) to the horizon.

From its nearly oviform shape, with the small end lowest, and the nearly uniform density of its contents, it may be predicated that its centre of gravity is nearly the centre of its mass. Now, every body tends to fall in a vertical line from the horizon, passing through its centre of gravity. A necessary condition for the support of any body is, that its centre of gravity be supported. This necessary condition is not fulfilled to the pregnant uterus by the pelvis. Consequently, the pelvis cannot be correctly described as *the* part supporting the uterus.

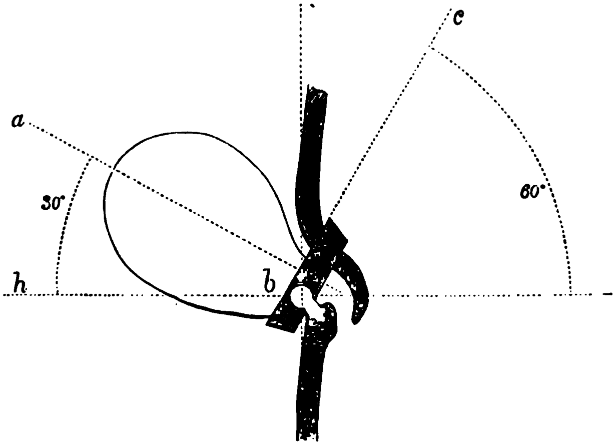

Fig. 1.

The pregnant uterus is supported on every side by the soft and elastic structures which everywhere surround it, as closely as if it were enveloped in a fluid.

Fig. 1. *a b* is the axis of the pregnant uterus and of the brim of the pelvis ; *b c* the inclination of the brim of the pelvis ; *h b* the horizon.

An especial force supporting it is probably to be found in the retentive power of the abdomen. No single part can be described as *the* part supporting it. But with this qualification, a near approximation to a correct statement of the conditions of its equilibrium is the following:—It reposes upon an inclined plane formed by the anterior abdominal wall, and is prevented from moving down the plane by the pelvis. To be at rest upon an inclined plane, a body must have a power exerted upon it to arrest its motion down the plane. In the pregnant female, this is the power exercised by the brim of the pelvis. The power required to resist the pressure down the plane (that is, the pressure on the brim of the pelvis), is to the vertical pressure on the inclined plane (that is, the pressure upon the anterior abdominal wall), as the height of the plane to its length. Now, in an inclined plane, which rises from the horizon at about an angle of 30°, the pressure on the plane itself (that is, on the anterior abdominal wall) must be greater than the pressure in the direction down the plane in the proportion of 87 to 50.* Thus it is found that the chief support of the uterus is the extensive, equable, soft, and elastic abdominal wall, not the comparatively limited, unequable, hard, and inelastic pelvic brim.

It is not necessary to point out at any length the evident application of these views in explaining the production of anterior obliquity of the pregnant uterus, or of pendulous belly. It is upon the anterior abdominal flap that the chief weight of the pregnant uterus reposes, and this part is consequently most liable to be morbidly displaced by this pressure. "There is an affection" . . (says Montgomery),* "constantly mistaken for, and treated as, local inflammation; I speak of a pain felt at either side, about the margin of the ribs, and arising from the dragging of the muscles at their insertions in that situation, especially of the oblique at their superior attachment." The same circumstances as throw the weight of the uterus on the anterior abdominal muscles, remove the main pressure of the organ from the brim of the pelvis, through which it would otherwise be more liable to prolapse, than it really is.

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* For this calculation, as for other kindnesses, I am indebted to Mr. J. Elliott, Lecturer on Natural Philosophy.

It is here necessary to point out that great misconceptions are entertained in regard to the position of the uterus, in different positions of the female. For instance, in attempting to disprove the gravitation-theory of the position of the fœtus in utero, to which we shall have afterwards to allude, it has been argued that that doctrine presupposes that the mother's body is in the vertical position, in order that the gravitation of the fœtal head may have the effect attributed to it, and that the horizontal position assumed during repose, or from other causes, ought to afford many more chances than the statistical results show, of the head assuming, by gravitation, other positions than the ordinary one. M. Dubois, who speaks of the "almost vertical direction of the uterus in the erect position," adds—"We cannot refrain from remarking, that there is a considerable number of women who, for the sake of their health, pass the greater part of their pregnancy in a position almost horizontal; and although even in this position the uterus may maintain something of its ordinary direction, nobody will believe that the slight inclination which its walls offer is sufficient to favour the descent of the head towards the uterine orifice. We are, nevertheless, not aware that in these circumstances the presentations of the head are less common than otherwise.*

---

* *Signs and Symptoms of Pregnancy*, p. 7.

Sir James Simpson, following Dubois, states this still more strongly. "The doctrine of gravitation" (says he) "presupposes that the mother's body is in the vertical or upright position, in order that the gravitation of the fœtal head may have the effect attributed to it. But during the hours of sleep and rest, her body is placed horizontally, and not vertically, and ought to afford many more chances than the statistical results show, of the head falling by mere gravitation into other positions and localities, than its usual and normal locality in the cavity of the cervix uteri. Besides, most practitioners have repeatedly seen patients restrained to the horizontal position for months before labour came on, without this position of the mother producing any deviation from the common position of the fœtus."† This argument, to have any force whatever, must imply that, in the vertical position of the female, the uterus is vertical, or nearly so; and in the horizontal or recumbent posture, horizontal, or nearly so. But this is very wide of the truth; nay, it is totally opposed to it. In the erect position of the female, the uterus is not vertical, but inclined to the horizon at a moderate angle of about 30° (see Fig. 1). "Whoever," says William Hunter,* "has any tolerable notion of the shape of the abdomen and situation of the cavity of the pelvis, must understand that the axis of the uterus is very far from the perpendicular line, its lower end being turned backwards, and its upper end, in proportion, turned forwards. This obliquity changes with the attitude of the body, and from many other causes. When erect, the weight of the uterus presses the fore part of the abdomen into a greater rotundity, and then the axis of the uterus approaches nearest to the transverse or horizontal line; and in a recumbent position, the contrary happens from a similar cause."

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* *Mem. de l'Acad. Roy. de Med.* tom. ii., p. 270.

† *Obstetric Works*, vol. ii. p. 88. Scanzoni (*Wiener Med. Wochen-*

"Owing" (says Montgomery) "to the oblique attachment of the pelvis to the spinal column, and the projection of the sacrovertebral junction coming in contact with the posterior surface of the uterus as it increases in size, and begins to ascend out of the pelvic cavity, that organ cannot rise perpendicularly, but its fundus is inclined forwards with its anterior surface

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schrift, Jan. 20, 1866; see *British Medical Journal*, March 17, 1866, p. 280) makes a like mistake, repeatedly assuming that the uterus is vertical.

* *On the Gravid Uterus.* Rigby's edition, p. 6.

lying against the peritoneal lining of the abdominal parietes—a relation of parts which continues unchanged throughout the whole period of pregnancy." *

"In the erect position of the body," says Robert Lee,† "especially if the abdominal muscles are relaxed the fundus uteri falls forward, so that the axis of the uterus forms a great angle with the spinal column and brim of the pelvis, and approaches the horizontal line." ‡ In the supine position we find the uterus (see Fig. 2) to be not horizontal, but inclined to the horizon at an angle greater than that assumed in the erect attitude of the female. This angle is between 60° and 30°, probably much nearer 60° than 30°. In

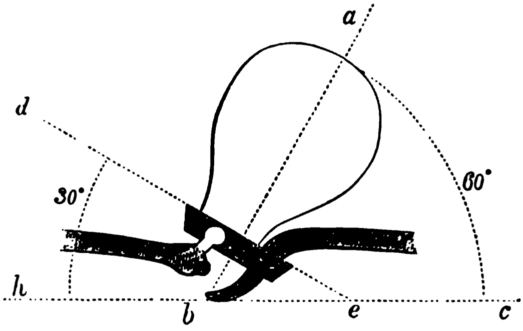

Fig. 2.

Fig. 2. *a b* is the axis of the pregnant uterus and of the brim of the pelvis ; *d e* is the plane of the brim of the pelvis ; *h c* is a horizontal line.

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* *Signs and Symptoms of Pregnancy*, p. 6.

† *Lectures on Midwifery*, p. 91.

‡ On this subject see some remarks by Scanzoni, *Chronische Metritis*, S. 54. Consult also *Stein. L'art d'Accoucher*, French transl. p. 147, etc.

this way it is evident that the uterus is more nearly vertical in the horizontal position of the woman than in the erect. In both cases it is in an oblique position; and by this beautiful arrangement, the uterus is preserved in an uniform condition, free from many statical variations which might otherwise affect it suddenly and injuriously.

It may be objected to the description of the pregnant female in the horizontal position that she is supposed to be lying on her back; and there can be no doubt that a woman may assume a horizontal position, not on her back, in which the axis of the uterus may be horizontal. But such position will only be an occasional one, not one long maintained. Besides, in the more advanced months of pregnancy, when the position of the child is gradually becoming fixed, its long axis corresponds to the axis of the uterus, and when the axis of the uterus is horizontal, the equilibrium of the fœtus will be stable, that is, the fœtus will have no mechanical tendency to change its position in any direction from that which it had before the horizontal position of the uterus was assumed.

The fact, that in none of the ordinary positions of the female is the uterus vertical, insures for the organ a considerable hydrostatical advantage. If the uterus were at any time vertical, then the lower part of the organ, the part least supported, and perforated by the os, would be liable to be subjected to the pressure of a column of liquor amnii of about twelve inches in height, or equal to the longest diameter of the uterus. But by inclining the organ from the vertical position, the vertical height of the column of water is diminished, and with it the amount of the fluid pressure on every part of the walls of the organ.*

Before leaving this portion of my subject, I wish to point out one practical application of it. In the third quarter of pregnancy, when the fœtus has acquired considerable firmness and weight, and before it has become at all closely adapted in form and dimensions to the uterine cavity, it may be made, by a very small amount of force, to bob about in the liquor amnii. Various positions of the female have been recommended as best adapted for successful repercussion or ballottement, as this bobbing of the fœtus is called. Numerous authors, especially those of the French school, advise the practitioner to place the woman in the erect position; others recommend the horizontal position on the side, on the back, with the shoulders raised, or on the knees and elbows. But a little attention to the configuration and attitude of the uterus will show that, on mechanical grounds, there is one position decidedly preferable to the others. This position is that of course where the operation can be best accomplished: in other words, where there is most room and freedom for the motion. In regard to the uterus itself, this is evidently its long diameter in a vertical direction. Its long diameter offers most space; and were it in any other than a vertical position, motion in it would be impeded, not only by the liquor amnii, but also by the fœtus's own friction on the wall of the uterus to which it gravitated, and on which it would, to some extent, move. Now, in what position of the female is the uterus placed, with its long

---

* "The extreme tenuity of the membranes (says Baudelocque, *System of Midwifery*, Heath's Translation, p. 289) may also occasion a premature delivery in women when the orifice of the uterus opens early, and especially in those who have already had several children; because in that case the membranes, being too weak to support the weight of the column of fluid which presses on that part, burst, and permit it to flow off, sometimes long before the proper period. We might cite a crowd of examples in proof of this assertion." I could also adduce cases in which I believe Baudelocque's explanation does to some extent hold good. But I must not omit here to point out that tenuity of membranes is no sign of their weakness. On the contrary, the amniotic membrane which has most tenuity has also most strength. For tenuity should be substituted the expression, want of power to resist a bursting pressure. This subject is more fully discussed in a succeeding part of this volume, on the Dynamics of Parturition.

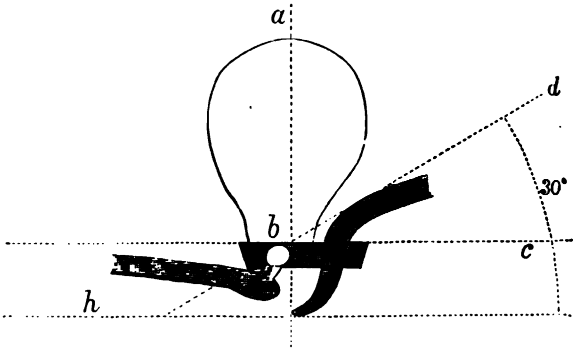

Fig. 3.

Fig. 3. *a b* is a perpendicular line in the axis of the pregnant uterus and of the brim of the pelvis ; *d h* is the axis of the woman's trunk raised at an angle of 30° to the horizon.

diameter vertical? We have already said that the organ developes itself forwards and upwards nearly in the direction of the axis of the pelvic brim. If, then, we place the brim, which, in the erect attitude, is inclined to the horizon at an angle of 60°, in a horizontal position (see Fig. 3), then the uterus will be vertical. For this purpose, the axis of the female, or at least the trunk, must be deflected to the same amount from the vertical line; that is, so as to form an angle of 30° with the horizon (see Fig. 3). Nægele, indeed, has pointed out that, in a position between that of sitting and recumbency, the superior plane of the pelvis will be horizontal in a well-formed woman. This can easily be effected, in actual practice, by propping the female with pillows on a couch. It is the position I have often found to be actually best adapted for the diagnosis of pregnancy by ballottement.

CHAPTER II.

THE NATURAL POSITION OF THE FŒTUS IN UTERO IN ADVANCED PREGNANCY.

To give point to my description of the position of the fœtus, I may first state that it is often described as being the opposite of that of the adult. It is said to carry its body vertically, but with the head placed undermost; in other words, it is said to stand upon its head. To show that these descriptions are entirely wrong, it is merely necessary to observe that the fœtus at the full time is closely adapted to the cavity of the uterus, and to recall to mind what has been already said in regard to that organ. It will thus be observed that in the erect and supine positions of the female, the fœtus, near the full time, lies obliquely to the horizon, that in neither attitude is it at all nearly vertical or horizontal, but in a position about midway; and that in the erect position of the female (see Fig. 1), it is more nearly horizontal than in the supine (see Fig. 2). The mature fœtus is nearly horizontal, in ordinary circumstances, only when the woman lies flat on the side. It is vertical, only when the trunk is inclined to the horizon at an angle of about 30°.

From this position of the adult fœtus in the axis of the brim of the pelvis, several minor advantages might be pointed out as accruing to itself. But we shall here merely observe that this fact is an important element in the mechanism of parturition, the fœtus being thus placed in the direction most favourable for enabling the uterine efforts to overcome the most important mechanical difficulty of labour, the passage of the brim.

In the same way as we have found the nearest (though far from absolutely) correct explanation of the equilibrium of the gravid uterus to lie in the mechanical principles of the inclined plane, so shall we find these similarly applicable to the case of the fœtus. As we described the uterus to derive support on all sides from the organs environing it, so we find the fœtus to be much more perfectly supported by the bland and warm liquor amnii, in which it neither *stands* nor *carries* itself, but reposes floating in almost perfect equilibrium. It is, however, specifically heavier than the liquor amnii. It therefore sinks in this fluid,* and it does so with an almost inappreciable force. In accordance with the doctrines of the inclined plane, this minute force is subdivided between the plane on which it lies, the anterior wall of the uterus, and the brim of the pelvis, the latter bearing the smaller share. The same hydrostatical circumstances of the floating fœtus which relieve the uterus almost entirely from the pressure at particular points of a body which might obstruct its circulation or otherwise injure it, do also, in the same degree, relieve the child from pressure which might be painful or irritating to it. By this beautiful arrangement, also, the cord is safe from liability to pressure which might be fatal to the child.* Again, the protection (says Dr. Arnott†) given to the tender fœtus by the liquor amnii is such, that a blow from without is expended on the surrounding water and cannot reach the fœtus. During labour these same waters perform important functions, which belong, however, to hydrodynamics, not to hydrostatics.

---

* At the full time sometimes so very little liquor amnii exists that it can scarcely be described as floating. The minuteness of the weight or force with which the fœtus generally presses on its supports can be understood by any one who has used the sign of ballottement. In performing this operation, the accoucheur, passing the finger per vaginam, generally succeeds in pressing the lower wall of the uterus against the presenting part of the child with some difficulty. With the point of his finger in this position, he jerks the presenting part of the fœtus. It immediately bobs up against the hand he has placed over the anterior abdominal wall, and drops down again on the point of his finger. If an accoucheur who has done this will try how he can move, with his

The head of the living fœtus at and near the full time is generally its lowest part, or that nearest the brim of the pelvis. The frequency of this, when compared with the other positions at or near the full time,

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hand similarly placed, a body weighing in air as many pounds as the fœtus does, he will then understand how nearly the fœtus must be in perfect equilibrium, and with how little of its weight it presses upon the uterus.

* I do not here enter into other provisions against this accident, the chief of which probably lies in the fact that the *besoin de respirer*, or the sensation of irritation analogous to it in the fœtus, excites it to motions which may relieve the cord from the asphyxiating pressure.

† *Physics*, vol. i. p. 688.

is such that it claims to be called the natural position of the child in utero. The evidence on which this important statement rests is twofold; for, first, we can by vaginal examination at this time feel the fœtal head in that place, diagnosing it from other parts of the child; and second, we know that (to use the words of the admirable W. Hunter), in the last two or three months of gestation the child is commonly so much straitened for room, and so compactly adapted to the oblong figure of the uterus, that it cannot change its general position, either by its own efforts or even by accidents happening to the mother.* It is on this last ground that accoucheurs do with justice adopt, as an index of the numerical frequency with which the head lies lowest in the end of pregnancy, the ascertained numerical frequency of presentations of the head at the commencement of labour.

It is here necessary to make a digression in order to consider the statements of the distinguished M. Dubois and his numerous followers, in regard to the position of the fœtus in utero when not at or near the full time. These statements are founded upon statistics of the presentations of premature children in the course of miscarriage or abortion. They have been chiefly adduced in the writings of Dubois and others, who have attempted to disprove the gravitation theory of the position of the fœtus in utero. The statistics show that the earlier the fœtus is expelled from the uterus, it is the less likely to present the head as the first part born. But by an unwarrantable and quite unsupported step in reasoning they have been represented as proving the position of the fœtus in utero. These statistics are valuable, simply as indicating the presentation, not before but only during parturition, of premature children, and if such is the case they are utterly valueless in the discussion of the question of the position of the fœtus before labour, or of its causes, into which they have been dragged.

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* *Gravid Uterus*, Rigby's Edit. p. 61.

Although the position of the premature fœtus is not the subject of discussion, yet it is interesting to add the following remarks by Hohl in confirmation of the view I have taken. "We have (says he) examined a series of unopened ova, and have always observed that the embryo reposing in the liquor amnii lies with the head lowest. We have, in a considerable number of autopsies of pregnant women, not advanced to the seventh month, never once seen the breech lying lowest, if the corpse had not been disturbed." In another place Hohl adds: "Stein, Grenser, and myself, have frequently, in premature labours, felt the head originally advancing first, and yet the breech gradually descended and was born first."*

I have already shown why the presentation, in early labour, of the fœtus at or near the full time, is a very exact and trustworthy indication of its position before labour, and that, in many cases, we can easily verify the identity of the presentation in labour with the part lying lowest before labour. But we have no evidence whatever that the presentation in a miscarriage or abortion is identical with the part lying lowest before miscarriage, and we have no means whatever of reasoning from the one to the other. On the contrary, the more round and globular shape of the uterus in the middle months of pregnancy (see Fig. 4), the non-adaptation of the child to the uterus, the great mobility of the child, and the greater abundance of liquor amnii in these months, are some of the mechanical elements in these early stages of pregnancy which render it almost absurd to reason from the presentation of a premature child during birth to its position in the uterus before the process of its expulsion commenced.

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* *Lehrbuch der Geburtshülfe*, S. 160 and 161.

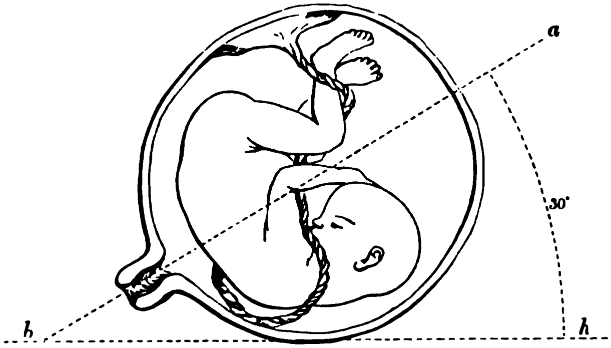

Fig 4.

There are no large collections of ascertained facts in regard to the actual ordinary position of the fœtus in utero in the middle months of pregnancy. We can

Fig. 4. *a h* is the axis of the pregnant uterus and of the brim of the pelvis ; *h h* is a horizontal line.

only reason in regard to it from comparing the position of the fœtal heart with the shape and site of the uterus, from feeling in some cases the position of the limbs or of the head in utero, from the questionable results of post-mortem examinations in these months, and from the statical knowledge which we can bring to bear upon it.

It is not my intention to enter into the whole subject of the causation of the attitudes and positions of the fœtus in utero. That would lead me into many questions very wide of a statical inquiry. I wish merely to point out the great and predominating influence in their production—namely, that power of gravitation which all bodies, alive or dead, must obey.

I have already incidentally shown that the main arguments used against the gravitation theory are founded in error. When rightly considered, they either afford no argument whatever, or else, evidence against the side they are intended to support. These arguments are based on the erroneous notion that the gravitation of the head to the lowest part of the womb can have effect only when the woman is upright, and is rendered ineffective when she is horizontal. They also are made to derive support from the reasonings founded upon the erroneous belief that the lowest part of the child, during a miscarriage, is with certainty lowest before the miscarriage commenced.

Another specious argument adduced against the gravitation theory requires only a few words to show its imperfection. If gravitation (it has been said) were the cause of the normal position with the head lowest, then this position should be found with more certainty when the gravitation of the head, from any cause, was proportionally greater than natural. In cases of intrauterine hydrocephalus, the child's head is larger and heavier than usual; and sometimes it is so to an excessive degree. But this increased gravitation (it is added) does not render head-presentations in these cases more common than usual, but the very reverse; as has been shown by Dr. Thomas Keith in his essay on this subject. "If the physical gravitation" (says Sir J. Y. Simpson)* "of the head of the child were the cause of the normal position with the head lowest, then this position ought to be found with more frequency and certainty when the gravitation of the head from any cause was rendered proportionally greater than natural; with less frequency and certainty, when from other causes the gravitation of the cephalic extremity of the infant was rendered proportionally less than natural. The very contrary, however, of all this is the truth. In cases of intrauterine hydrocephalus, the child's head is larger and heavier than usual; and sometimes it is so to an excessive degree. But this condition of the head, this increased preponderance and gravitation of it, does not render head presentations in these cases more common than usual, but the very reverse." Now this argument, when justly applied, is made to tell most decidedly in favour of the gravitation theory. The altered circumstances of a hydrocephalic fœtus have been altogether misapprehended. The hydrocephalic head, although truly much larger and heavier in air, is probably lighter and more buoyant in water. In this question we have to do with hydrocephalic children only while immersed in liquor amnii. The larger the head is in these circumstances, the lighter and more buoyant it is, and the other extremity of the child is proportionally heavier. The fluid effused in hydrocephalus* is specifically lighter than brain, and therefore renders the head more buoyant than it is under natural circumstances. It is also in all probability specifically lighter than liquor amnii, and consequently its accumulation will have a more decided effect in elevating the head. In this way it is demonstrated that the four times greater frequency of preternatural presentations, in cases of hydrocephalic children, proves rather than disproves the influence of gravitation in deciding the position of the fœtus.

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* *Obstetric Memoirs*, etc., vol. ii. p. 90.—Extracted from *Monthly Journal of Medical Science* for 1849.

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* I lately had an opportunity of procuring hydrocephalic fluid from a living child still in utero. I had drawn off the water in order to facilitate delivery ; and I am indebted to Dr. George Wilson for a careful examination of the fluid. He found its specific gravity at 60° Fahr. to be 1007·9. The specific gravity of fœtal brain I do not know, but that of the adult varies between 1030· and 1050· See Dr. Skae's Paper on the "Weight and Specific Gravity of the Brain in the Insane"—*Monthly Journal of Medical Science*, October 1854. In an experiment I lately made on a fresh fœtus at the full time, which had been exposed to the air for about 24 hours, I found the specific gravity of the entire body to be nearly 1050 at 44° Fahr. ; and that of the head, when separated, to exceed that of the headless trunk. The specific gravity of liquor amnii of a premature ovum is stated by Dr. Rees at 1008·6 (see *Churchill's Midwifery*, 3d ed. p. 93). But it, no doubt, varies in different cases, and probably in the same case at different times. Professor Turner found the specific gravity of hydrocephalic fluid from a female, æt. 17, drawn off during life, to be 1009 ; in another case of chronic hydrocephalus, drawn off after death, to be 1004·2 at 59° Fahr. Hoppe (Virchow's *Archiv.* 1859, s. 391) states the specific gravity of fluid removed after death in a case of hydrocephalus to be 1001, and in another to be 1005.

Again, it has been argued that anencephalic children, with the whole brain and arch of the cranium wanting, are still often found presenting naturally, whereas it has been thought that this great deficiency should render presentations of the head less frequent than natural in these cases.* What is the fact, as to the greater or less frequency of the presentations of the head in these cases, I do not know.

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* The erroneous and inconclusive reasoning of authors on this subject is well illustrated in their discussing the position of anencephalous monsters.

"Let us add (says Dubois, *Mem. de l'Acad. Roy. de Med.* tom. ii. p. 270) that, if the laws of gravity had so great an influence upon the position of the fœtus, then the anencephalous, deprived of the greater part of the brain, should offer almost constant exceptions to the ordinary rule. Their head being much lighter, should, in fact, occupy the upper part of the uterus, and their pelvic extremity, drawn by its greater weight, should be approximated to the uterine orifice; but such is not the case. We cannot, however, deny that the cephalic extremity of these fœtuses presents first in labour less frequently than is observed with those who are well formed. It is not impossible that the preponderance of the pelvic extremity may have some effect in these anomalies, but we are of opinion that it is only an accessory cause."

We find Sir J. Y. Simpson agreeing with Dubois in this argument. He says (*Memoirs*, etc., vol. ii. p. 90), "If the physical gravitation of the head of the child were the cause of the normal position, with the head lowest, then this position ought to be found . . . with less frequency and certainty, when from other causes the gravitation of the cephalic extremity of the infant was rendered proportionally less than natural. .

But whatever it may be, the argument is very inaccurately used. For even without the head altogether, the upper end of the trunk is the heavier. "Without descending to minutiæ"* (says W. Hunter) "it must be observed that the trunk is very small in proportion to the head, and the lower part of the body when compared with the upper; thus, the upper part of the trunk of the body is small with regard to the head, the lower part of the trunk is small in proportion to the upper part, and the lower extremities are small in proportion when compared with the arms."

I lately tried the experiment of floating in a solution of salt, of nearly its own specific gravity, a fresh decapitated fœtus, and found that it floated obliquely, with the neck very decidedly lowest. But before any argument, applicable to the case of healthy pregnancy, can be derived from observing the presentations of acephalous or anencephalous children, it is necessary to take notice not merely of the presentation in such cases, but the shape of the uterus, the quantity of liquor amnii, and of any abnormal circumstances in such abnormal pregnancies affecting the statical conditions of the fœtuses. Till this be carefully done, we can base no reasonings upon the presentations of anencephalous children whose statical circumstances we do not know.

Anencephalic fœtuses, with the whole brain and arch of the cranium wanting, are still often found presenting naturally. I have been present at the birth of three anencephali that had reached the full term of pregnancy. All of the three presented with the deformed and diminished cephalic extremity over the os uteri."

* *On the Gravid Uterus*, p. 62.

Further, it has been attempted by Dubois and others* to show that the position assumed by the fœtus when immersed in a fluid, or rather when allowed to fall through a mass of water, is inconsistent with the gravitation theory. It has been shown in these ill-devised experiments that the part of the fœtus which reaches the bottom of the vessel first is not the head, as these authors fancy should be the case if the gravitation theory were correct, but is the shoulder. But the fact is, that the line of descent through a fluid of nearly equal specific gravity with the body descending, is, in the case of the fœtus, as in all other bodies, not a vertical line in the long axis of the body, drawn from what is the absolutely heaviest part, but a vertical line from the centre of buoyancy to the centre of gravity. (See Fig. 5.) The same fact is expressed in the hydrostatical principle, that a body floating and immersed is in stablest equilibrium when the centre of gravity is vertically below the centre of buoyancy,—*i.e.* the centre of gravity of the fluid displaced. These very experiments, and numerous other facts, show that the centre of gravity is much nearer the cranium in the fœtus, and especially when in the fœtal attitude, than it is in the adult; that it is, in fact, near the upper part of the back, and nearer the posterior than the anterior surface of the child.* The centre of buoyancy of the fœtus will be very nearly the centre of the oval figure which encircles the fœtus. (See Fig. 5.) Both these centres will be liable to constant variations, with the varying dimensions and specific weights of different parts of different fœtuses, and with the varying attitudes of

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* "When a human fœtus of the latter months," says Dr. Simpson, "is placed experimentally in fluid, in descending through the fluid the head does not turn and fall first to the bottom of the containing vessel, as the theory of gravitation takes for granted that it would. Thus, if a dead new-born fœtus be plunged into water, contained either in a vessel shaped like the uterus, or in a large bath, the part which gravitates and strikes the bottom of the vessel or bath first, is the back or scapula, and not the head. M. Dubois first pointed out this fact, as the result of numerous experiments which he had made on fœtuses from the fourth to the ninth month; and in several instances in which I have repeated the experiment, I have found the same result."

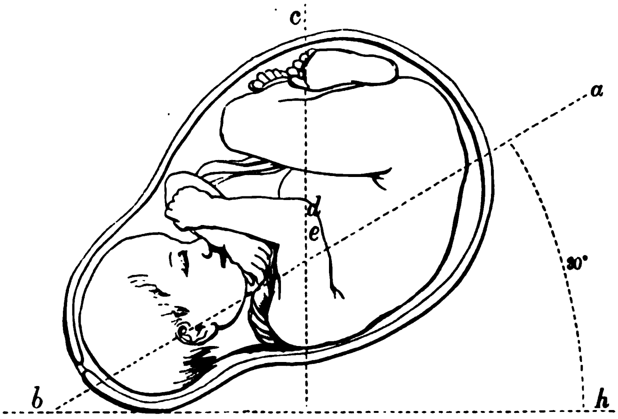

Fig. 5.

Fig. 5. *a b* is parallel to the axis of the pregnant uterus and of the brim of the pelvis; *c d e* is a perpendicular line; *e* represents the centre of gravity of the fœtus; *d* the centre of flotation.

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* "About the middle of pregnancy (says Scanzoni), in consequence of the rapid development of the body of the fœtus, the centre of gravity falls from the head to the upper part of the thorax."—*British Medical Journal,* March 17, 1866.

the parts of the same fœtus. But allowing for every variation, it is evident that if these two centres be, in the case of the fœtus, placed in the same vertical line, the fœtus will be inclined obliquely to the horizon, as we find it to be in its ordinary position.*

The experiments of Dubois and Simpson, involving, as they do, rapid motion in falling through water, are fitted to afford results of a very small degree of value or reliability. So far as they go, they point to the fact of the descent of the fœtus, shoulder lowest, or, to speak more correctly, following the shoulder. In an experiment I lately performed with a fresh, still-born, adult fœtus, retained by threads in the fœtal attitude, I found it to poise itself in an oblique direction, head lowest as in Figure 5, in a solution of salt nearly its own specific gravity. This position evidently corresponds closely to the ordinary uterine position.

This argument against the gravitation theory, it thus appears, has been quite misapprehended, and a more correct investigation of the circumstances reveals to us here, as everywhere else, the beautiful arrangements of nature, whereby the fœtus reposes, in its sojourn in the womb, in that position which is easiest and securest to the mother and to itself,—in a position, indeed, which requires no effort whatever on its part for its adoption and maintenance.

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* In an excellent paper, Battlehner (*Monatsschrift für Geb.* iv. Band 1854) anticipates more than one of my demonstrations and arguments. It unfortunately did not come under my notice till long after my original paper was read to the Medico-chirurgical Society (December 20, 1854), and published. Battlehner acutely criticises the ill-devised and well-known experiments of Dubois, in which he lets a fœtus fall through a fluid, to observe what part first reaches the bottom. Speaking of this experiment, he says that the back, or one of the shoulders, first reaches the bottom of the vessel, showing that the centre of gravity is not in the head, but in a part of the trunk nearly adjoining it.

The only other objection to the gravitation theory, which I think deserving of mention, is founded on the fact that dead or putrid children present the head as the lowest part in labour less frequently than the living; and it must be admitted that the death of the fœtus does not remove it from the agency of gravitation. From these propositions the conclusion is jumped at, that mere gravitation cannot account for this difference in ordinary position of the putrid and the living, for it is evident that gravitation should affect live just as it does dead matter. But here it has only to be pointed out that the major proposition of the syllogism is an assumption, and that this argument is a reasoning in the dark.* The assumption is one very highly improbable, namely, that a dead or rather a putrid child, is in the same condition, *quoad* gravity, as a living one.

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* Sir J. Y. Simpson follows Dubois in this line of argument. I shall quote the words of the former, in illustration of it (see *Memoirs and Contributions*, vol. ii. p. 91):—

"When the child dies *in utero*, it still continues to be subjected to the same physical laws as when it is alive. The mere death of the fœtus does not in any way remove it from the agency of gravitation. When all its vital actions have ceased, the body should in fact be more subject than heretofore to all influences, such as gravitation, which are merely physical in their character. The dead infant ought, therefore, as frequently as the living, to have its head placed as the presenting part over

We know that in the adult, at least, the circumstances of death and putridity produce great changes *quoad* gravity. What the changes are in the fœtus in similar circumstances we do not know. Till we do know, we must be content without attempting to reason as if we did.

Before proceeding, with correctness, to argue from the fact of frequent mal-presentations of dead children to the causes of the ordinary presentation of the head, we must take carefully into account not only the new circumstance of death in these cases, but also all other circumstances in pregnancies with dead children, which are, or may be, statically different from the circumstances of healthy pregnancy. Among these are prematurity, quantity of liquor amnii, the shape of the uterus and the shape of the fœtus. I have already said that those who use this argument have not directed their attention even to the altered statical relations produced by death and putridity of the fœtus. So far as my observations (to be subsequently related) go, they tell against the argument of the opponents of the gravitation theory. Take one experiment meantime as an

the *os uteri*, provided the mere physical gravitation of the head were the reason and cause of that position. Experience, however, amply proves that this is not the fact. In other words, experience proves that malpositions of the child, or the presence of other parts than the head at the cervix and os uteri, is in the latter months a far more frequent occurrence when the child is dead than when it is alive. . . . The position of the fœtus *in utero*, with the head lowest, is a vital action, etc. . . What (he subsequently adds) is the nature and character of the muscular action by which the fœtus assumes and maintains its position in utero with the head lowest example: I floated in a solution of salt, of about its own specific gravity, a six months' fœtus, born dead, and with the scarf-skin peeling off. The position it assumed was exactly the reverse of that taken by the fresh child at the full time, and which I have already described. The putrid fœtus floated with its long axis directed obliquely to the horizon, and with its head highest. In labour it presented the breech.

The position of the fœtus at the full time is, in the great mass of cases, fixed and determined about the end of the seventh month of pregnancy. This arises from the fact that about that time the size and shape of the uterus become so nearly and closely adapted to the size and form of the fœtus, that it cannot change the position of its trunk in any material degree. After this time the position of the fœtus must be one determined by gravitation, for it is impossible to conceive its reposing in any other.

All the knowledge we possess of the position of the fœtus, after it has entered the second half of pregnancy, leads us to believe that its head lies ordinarily lowest. Before the seventh month it is still capable of having its position in utero changed, by changes merely in the attitude of the mother, and probably it possesses the power of effecting temporary changes, at least, by its own unaided movements. But the fœtus is generally in a state of repose, and not producing motions in its limbs or body. In this state of repose, in a fluid of nearly its own specific gravity, it is impossible to conceive of its maintaining any position but under the influence of gravity. Its position must at all times be mainly, if not entirely, caused and determined by statical circumstances. It is quite conceiveable, that while still comparatively free in the uterus it may, by virtue of its very easy mobility in the dense liquor amnii, change its position. If this occur at a time when its dimensions are beginning to approximate to those of the uterus, having overcome some resistance of the uterine walls by the force of its own muscular efforts, or otherwise—as by accidents to the mother—it may not gravitate back to its old and ordinary position; and thus a preternatural presentation may be produced. The uterine walls are everywhere smooth and glabrous, and rounded; and the fœtus lies in this cavity with its legs, its chief organs of locomotion, elevated; circumstances which appear to render its maintenance of any position but that of gravitation a greater feat than ever was performed by a rope-dancer. With all the advantages of its new circumstances, the child after birth cannot assume or maintain any position. How much less could it be expected to do so in the uterus, and under circumstances so disadvantageous for the fulfilment of such a function. Those authors who, with Dubois, strive to prove that the position of the fœtus is determined by its own motions, have first to prove that it could maintain any position whatever against gravity, without such constant efforts as voluntary muscles are incapable of, and of the actual presence of which no evidence can be furnished.

CHAPTER III.

INTENIBILITY OF THE THEORIES OF THE POSITION OF THE FŒTUS IN UTERO, REQUIRING MUSCULAR MOVEMENTS.

It appears to me that obstetricians, especially Dubois and Simpson, and more lately Scanzoni, commit a very great error in their philosophising on this topic, when they mix up the study of the natural position with the study of malpositions. There can be no objection to using all the knowledge we possess in regard to malpositions, so as to make it throw light on the cause of the natural position. But that is very different from the proceedings of the authors referred to in their arguments from the circumstances of a malposition to the circumstances of a natural position. I have already shown how erroneously they have proceeded in fact. But what I at present intend is to point out the advantage of first settling the cause of the natural position. It is the commonest; it is most available for study; its conditions are well known; its discovery and full explanation will probably lead to an easy settlement of all disputes regarding the causes of malpositions; its separate discussion keeps the argu·ent to a well-understood field, and keeps the disputants from danger of much bad logic. The conditions of the fœtus in malpositions are ill known, have been little studied; and all arguments based on the history of malpositions require careful scrutiny, because very few general statements can be truly made regarding them.

The theories of Dubois and of Simpson are nearly identical, and accordingly the latter has availed himself of the ready-made arguments of the former, to support his variation on Dubois' theory. Both theories demand, as an essential element in the causation of the natural position of the fœtus in utero, its life and use of its muscular power. Dubois resorts to instinct and the will of the fœtus to bring the muscles into activity. Simpson asserts that their activity is a reflex phenomenon. Since their publication, these theories have been and are extensively adopted by teachers. Their intenibility is easily shown by the following consideration, which also leaves no room for entertaining anything else than gravitation as the main cause, however much muscular motions of the uterus or fœtus may occasionally interfere to produce temporary or permanent variations of the ordinary phenomenon. That muscular activity does so interfere to a great extent there can be no doubt.

Did the fœtus in utero maintain any position other than that of gravitation, it must be making incessant efforts to do so. A moment's rest will insure its falling into a position given it by gravitation. It does not exert incessant activity, therefore its maintenance of any position different from one produced by gravitation is impossible.

When the fœtus is immature and its liquor amnii abundant, the motions of the mother will always secure its return in a short time to the ordinary position of stable equilibrium, which I have already described. The position of its stable equilibrium, relatively to the mother, will vary according to the position of the mother.

About the seventh month of pregnancy, when the fœtus begins to find itself in a comparatively narrower chamber, its movements, whether active or passive, will have less scope; but, movement once effected, its return to the position of stable equilibrium of earlier months may be less certainly expected, because it may come to rest in a position of gravitation without resuming the position of stable equilibrium when floating more freely.

In the last months of pregnancy, if the fœtus be accidentally and violently displaced by its own movements or otherwise, it will probably never resume its original position, as it will have stability in its new position, and may not again be subjected to displacing violence.

At all times, and under all circumstances, it must assume a position of gravitation as soon as it ceases to make efforts, whether voluntary, instinctive, or reflex.

This argument is sufficient to show the intenibility of the theories of both Dubois and Simpson. But to the latter another fatal objection can be taken. Dubois' theory is, that under some impulse the fœtus takes its natural position in utero. Sir James Y. Simpson's theory is, that the uterus having a certain ovoid form, the fœtus assumes an ovoid form and adapts itself to the shape of the chamber in which it lies. Now here there is a very grave error in reasoning, probably also in physiology.

The error in reasoning consists in assuming, as an essential part of the argument, that the uterus takes an ovoid shape independently of its contents. This requires to be proved. Till it is proved, no further step in the argument can be taken, and all the author's conclusions remain without any basis whatever.

The probable error in physiology is in the assumption that the uterus assumes a shape independently of the ovum or fœtus. It appears to me that the very reverse is nearer the truth. No doubt nature provides for the development of the uterus into its ordinary shape at the full time. Of this we find indications even in the anatomy of the unimpregnated organ. But, so far as I know, the weight of authority is in favour of the opinion that the ovum or fœtus gives laws of shape to the uterus, and not *vice versa,* as Sir James Simpson assumes. The weight of facts, in natural and morbid conditions, is on the same side. For the uterus has no difficulty in adapting itself to great distension, to the shape of twin ova, to the shape of deformed women, or to the shape of a collection of fibrous tumours.

Sir James Simpson assumes the ovoid shape of the uterus as independent of its contents, apparently in forgetfulness that it is a groundless assumption. He does not take pains, as he should have done, to demonstrate it. I have made no regular scrutiny of anatomical or physiological works with a view to finding the opinions of authors; but in the course of my reading I have met with several expressions of opinion on this point which I shall quote. They will be observed to be unanimously in favour of a view entirely opposed to the assumption on which the reflex theory of the position of the fœtus in utero is based. Verheyen in his work on anatomy, published at Brussels in 1710 uses the following distinct words: "In gravidis se contentis accommodat non vice versa." William Hunter, in his anatomical description of the human gravid uterus, describes the plastic state of the uterus as making it adapt its figure to the circumstances of the child within, and vary as these change. He says its figure differs from the regular oviform from a variety of accidental causes, as it adapts itself to the neighbouring parts, to the attitude of the body, and to the position of the contained child. "We not only" says he "in dead bodies see the parts of the child making a variety of different projections on the outside of the uterus, but in the living body all the same variety is frequently manifest to the touch, on examining the outside of the abdomen." Dr. Read, in his work on Placenta Prævia (p. 97), makes the following statement regarding the form of the uterus: "In normal presentations, at the period of its utmost development, the general outline of the uterus is pyriform, the base being above, and the apex resting in the cavity of the pelvis. When the breech presents, it becomes almost globular; and in transverse presentations, the long diameter is nearly at right angles to the axis of the pelvis; proving very conclusively, that the shape of the uterus at the termination of gestation is not the result of development, but depends upon the position of the contained fœtus." Lastly, I quote briefly from Joulin who, in his *Traité Complet d'Accouchments* (p. 1006) says, "The uterus in pregnancy is a membranous and pliant sac, which has no other form than that which is impressed upon it by the situation of the infant."

"The reason," says W. Hunter, "why the child's head is commonly downwards may be supposed to be this: the child is specifically heavier than the liquor amnii, and therefore, in the various attitudes of the mother, is always in contact with and supported upon the depending part of the uterus. This in the more common attitudes is the cervix uteri. The child's head and upper part of the trunk contain more matter in proportion to their surface than the lower part of the body; thence the head will more generally fall down to the lower part of the uterus. And for the same reason, were a child to be dropped into deep water, in various postures and directions, its head would always first reach the bottom. But the motion of the head itself, either alone or in co-operation with the attitude of the mother, may sometimes turn the head towards the fundus uteri."*

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* Rigby's Edition of Hunter on the *Human Gravid Uterus*, p. 60.

CHAPTER IV.

POSITION OF THE PREGNANT FEMALE.

THE researches of Weber, of Meyer,* and of others, have shown that in the erect attitude of both sexes in ordinary circumstances, the body is balanced upon the two ilio-femoral articulations, the atlanto-occipital and sacro-lumbar articulations being nearly in the same transverse-vertical plane with the ilio-femoral articulations; the knee-joints and ankles being also in it. This adjustment and balancing of the trunk upon the ilio-femoral articulations must, in the impregnated female, be gradually disturbed and changed more and more till the full period of pregnancy arrives. At this time the weight of the entire gravid uterus and contents is superadded to that of the anterior parts of the body, which, in the virgin state, balanced the posterior parts upon the ilio-femoral articulations, the fulcrum of the pelvic lever. To restore the balance a corresponding amount of weight must be added to that upon the posterior arm of the pelvic lever. The weight added in front is probably about from twelve to fourteen lbs. avoirdupois,† and to restore the equipoise a corresponding amount of the upper part of the trunk and of the head must be moved backwards, so as to increase the weight behind. These theoretical observations must have been frequently confirmed by every observant eye in actual life. It is then to be remarked that the protuberant abdomen is rendered apparently more so by the attitude of the woman being as if excessively erect.* To the necessity for this new adaptation, the increased anterior development and weight of the mammæ will contribute. In small and especially short-bodied women, this change is most evident, a circumstance easily explained by the greater prominence forwards in such

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* Müller's *Archiv.* 1853.

† Ramsbotham (*Obstetric Medicine*, p. 82) calculates the weight of the contents of the gravid uterus at between nine and ten pounds.

In accordance with this notion I have drawn up the following approximative statement of the probable average weight of the gravid uterus and contents :—

	lbs.	oz.
Fœtus	7	0
Placenta Cord and Membranes . .	1	5
Liquor Amnii	1	8
Uterus as weighed post partum . .	2	0
Blood in Uterine Sinuses, etc., say .	1	0
	12	13

For Dewees' estimate, see his treatise on the *Medical and Physical Treatment of Children*, p. 16.

* "From this obliquity of the uterus (says Montgomery, *Signs and Symptoms of Pregnancy*, p. 7) the direction of the centre of gravity is changed, and instead of falling between the feet, it falls in front of them, in consequence of which the person has an inclination to fall forwards, and in order to prevent this is under the necessity of throwing back the head and shoulders, and assuming that pompous air which is so often unjustly attributed to a wish to make a display of her condition."

of the uterine tumour, and by their not being able otherwise to adapt the trunk to the new circumstances, as can be done with greatest facility in tall or long-bodied women. Some few women, especially those last alluded to, are enabled to conceal to a certain extent their gravid condition by not assuming this change of attitude, nature securing by a different mechanism of the trunk the equipoise of the trunk upon the lower limbs.

In the mechanism of the erect position, the centre of gravity of the parts above is nearly vertically over the ilio-femoral articulations, or points of support. The addition of the weight of the gravid uterus and contents in front, will move this centre forwards from its position in the unimpregnated state. This is prevented, however, in most cases, by the corresponding motion backwards of the upper portion of the trunk. In some women, either from voluntary or involuntary causes, this last motion does not take place. The equipoise of the body upon the ilio-femoral articulations, or restoration of the centre of gravity to its ordinary position above these points of support, must be otherwise effected. This can evidently be done only by moving forward the supports, so that they may be adjusted to the new site of the centre of gravity in this class of pregnant females, and this may be effected by diminishing the angle which the pelvis forms with the horizon. In this diminution of the inclination of the pelvis, forward movement of the ilio-femoral articulations takes place, and the equipoise is restored without any backward motion of the upper part of the trunk. This change in the pelvis is analogous to that taking place in old age, when the forward stoop is counterbalanced by it.* The same is also observed in youth, when a forward stoop is produced, as in the course of some affections of the vertebræ.

I have described these two distinct mechanical arrangements for the equipoise of the pregnant female, as occurring in different classes of pregnant women, merely to facilitate description. It is evident that in any case a certain amount of the one may be adopted as the complement of the other. But in many women the distinction and separation appears really to be maintained. In casually observing numerous pregnant females in this point of view, I have remarked two very characteristic classes. In the first class, the women are generally not tall in stature, have the upper part of the body much inclined backwards, and the hips also prominent, indicating a probably considerable obliquity of the pelvis. In the other class, the women if not tall are generally slender. In them the upper part of the trunk does not appear to be inclined backwards, and the hips are generally flat.

In quitting this subject, I would wish to remark that in framing our views as to the inclination of the pelvis, we must be guarded against the error of arriving at conclusions too absolute and strict. For it appears to me that the inclination of the pelvis is liable at all times to vary in the same manner, as I have described it sometimes to do in pregnant females. Moreover, the observations of Zaglas, Wood, and others, show that the innominate bones are far from being immovably fixed to the sacrum,* and are capable of certain motions upon it, which are capable of being turned to advantage in establishing the equilibrium necessary for the erect position.

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* The view here propounded is probably not applicable to all cases, but it has been adopted by Braun. See his *Lehrbuch der Geburtshülfe*, 1857, S. 11.

The last statical consideration which I shall dwell upon, in connection with the position of the pregnant female, is one of practical importance, and which may be turned daily to account in actual practice.

I have already pointed out that the attitude of the gravid uterus is inconsiderably changed by the woman shifting from the erect to the supine posture, or *vice versa.* But though this be the fact, as regards the mere attitude of the uterus, yet great changes take place in it, in regard to its liability to vascular congestion, and other phenomena connected therewith. When the woman is recumbent the blood is easily returned to the heart from the uterine sinuses, and there is little danger of vascular engorgement taking place. When the woman stands the return of blood is not so easily effected, it having now to overcome the increased resistance involved in the change of the position of the vena cava, from being nearly horizontal to being nearly vertical: and it is to be remembered, as a further disadvantage to the circulation in this point of view, that the uterine veins have no valves. Of this circumstance clinical illustrations abound in the practice of obstetrics. A woman of delicate fibre, and easily made to miscarry, may require to maintain the horizontal position for the whole, or a part only, of the period of pregnancy. In some such cases the assumption of the erect attitude for the shortest time induces engorgement of the uterine sinuses, separation of the placenta, hemorrhage, and abortion, or miscarriage. Hemorrhage, again, is induced in some cases of ulceration and polypus, only by continuation in the erect attitude; in the same way colour often re-appears in the discharges after delivery, on the first getting up; and all uterine hemorrhages are aggravated by the erect attitude. To the same cause also are to be attributed many inflammatory affections of the uterus, and many of the uterine pains attending disease in this region.

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* See *Dublin Quarterly Medical Journal* for August 1854, for a full discussion of this subject, and a subsequent chapter of this book.

CHAPTER V.

ON THE MODE OF PRESENTATION OF DEAD CHILDREN IN LABOUR.

It is well known that death of the fœtus in utero is a frequent cause of malpresentation. In other words, dead fœtuses much more frequently present the breech or shoulder than the living. This is easily proved by statistics. For instance, "during the seven years that Dr. Collins had charge of the Dublin Lying-in Hospital, 16,654 children were born within the house. Of these 16,654 children, 15,533 were born alive, and 1121 dead. Of the 1121 dead children, many no doubt died during labour; but 527 of them were in a putrid state, and consequently may be correctly regarded as having perished in utero, and before labour commenced. Among the 15,533 children born alive, 278 presented preternaturally, or 1 in 57; 250 presented by the pelvic extremity, or 1 in 62; 28 presented by the upper extremity, or 1 in 555; 15,255 presented by the head, or 98 in 100. On the other hand, among the 527 children born putrid, as many as 94 presented preternaturally, or 1 in 5; 88 presented by the pelvic extremity, or 1 in 6; 6 presented by the upper extremity, or 1 in 88; 433 presented by the head, or 83 in 100."*

Presentations of Living and of Dead Children.

Presentations.	Living.	Dead.
Head	98 per cent . . .	83 per cent.
Breech, etc. . .	1 in 62	1 in 6.
Shoulder, etc. .	1 in 555	1 in 88.

It is necessary to observe that in these statistics there are two sources of error to be taken into account; for to be exact in comparing the two classes of dead and living fœtuses as to presentation, we ought to be sure that both were at the same period of pregnancy, seeing that we know prematurity to be a frequent cause of malpresentation: and again, only the putrid are included; now putridity at birth is certainly a pretty sure indication that the child was dead before labour commenced, but this sign assumed as the test of death before labour commenced will likely exclude not a few that were really so, and which should be included among the dead, if the statistic is to be exact. But, after allowing for these two sources of error, the great comparative frequency of malpresentation of children born dead is evidently demonstrated.

The valuable observations of Dubois carry us a step further. They demonstrate that among premature children presentations of the head are much less frequent if the fœtus is dead than if it is alive. For instance, of 119 children born during the seventh month of pregnancy 73 were expelled alive, 46 dead.

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* Simpson's *Obstetric Memoirs*, etc., vol. ii. p. 91.

Of the 73 expelled alive, 61 presented the head, or at the rate of 83 in 100; 10 presented the breech, or 1 in 7; 2 presented the shoulder, or 1 in 31. Of the 46 expelled dead, only 21 presented the head, or at the rate of 46 in 100; 21 presented the breech, or about 1 in 2; 4 presented the shoulder, or 1 in 11.

Presentations of Living and of Dead Premature Children.

Presentations.	Living.	Dead.
Head	83 per cent . . .	46 per cent.
Breech, etc. . .	1 in 7	1 in 2.
Shoulder, etc. . .	1 in 31	1 in 11.

As in the former statistic, so in this, there lurks a source of error in our ignorance as to the exact number of the dead born that were dead before labour commenced. This is as important here as in the last statistic; for in both cases it must be evident that only deaths before the commencement of labour can influence the presentation. Deaths occurring after the child has assumed its position in labour can have no influence on the presentation; and if included in a statistic such as the above, will tend to mislead us in an inquiry as to the influence of death.

In the last statistic given there is another source of uncertainty which deserves to be pointed out. This has been strongly stated already.* The statistics show that the earlier the fœtus is expelled from the uterus, it is the less likely to present the head as the first part born. But by an unwarrantable and quite unsupported step in reasoning, they have been represented as proving the position of the fœtus in utero. These statistics are valuable, simply as indicating the presentation not before, but only during parturition of premature children; and if such is the case, they are utterly valueless in the discussion of the question of the position of the fœtus before labour, or of its causes, into which they have been dragged. I have already shown why the presentation, in early labour, of the fœtus, at or near the full time, is a very exact and trustworthy indication of its position before labour;* and that, in many cases, we can easily verify the identity of the presentation in labour with the part lying lowest before labour. But we have no evidence whatever that the presentation in a miscarriage or abortion is identical with the part lying lowest before miscarriage, and we have no means whatever of reasoning from the one to the other. On the contrary, the more round and globular shape of the uterus in the middle months of pregnancy, the non-adaptation of the child to the uterus, the greater mobility of the child, and the greater abundance of liquor amnii in these months, are some of the mechanical elements in these early stages of pregnancy, which render it almost absurd to reason from the presentation of a premature child during birth, as to its position in the uterus before the process of its expulsion commenced.

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* See page 19.

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* The evidence on which this important statement rests is twofold: for, first, we can by vaginal examination at this time feel the fœtal head in that place, diagnosing it from other parts of the child; and, second, we know that (to use the words of the accurate W. Hunter) in the last two or three months of gestation the child is commonly so much straitened for room, and so compactly adapted to the oblong figure of the uterus, that it cannot change its general position, either by its own efforts, or even by accidents happening to the mother. It is on this last ground that accoucheurs do with justice adopt as an index of the numerical frequency with which the head lies lowest in the end of pregnancy, the ascertained numerical frequency of presentations of the head at the commencement of labour.—*Edin. Med. and Surg. Journal*, January 1855.

These evidently just observations may require to be considerably modified by researches such as we now propose to relate. These observations will show a considerable amount of coincidence between the presentation of premature children and the part of their bodies which has a tendency to get lowest while floating. But after all, the above remarks will be true as long as our present means of research remain as they are. It might naturally be expected that the maceration and softening of the child's body after death would specially increase the difficulties of reasoning, from the position in labour in such cases to the position before labour; but this circumstance would probably find an equivalent in the active motions of a living child at the commencement of a miscarriage.

These facts, in regard to the mode of presentation of dead children, have hitherto been erroneously regarded as proving that gravitation could not explain the usual presentation of the child's head. "When the child dies in utero," it has been said,* "it still continues to be subjected to the same physical laws as when it is alive. The mere death of the fœtus does not in any way remove it from the agency of gravitation. When all its vital actions have ceased, the body should, in fact, be more subject than heretofore to all influences, such as gravitation, which are merely physical in their character. The dead infant ought, therefore, as frequently as the living, to have its head placed as the presenting part over the os uteri, provided the mere physical gravitation of the head were the reason and cause of that position. Experience, however, amply proves that this is not the fact. In other words, experience proves that malpositions of the child, or the presence of other parts than the head at the cervix and os uteri, is, in the later months, a far more frequent occurrence when the child is dead than when it is alive. . . . The position of the fœtus in utero, with the head lowest, is a vital action."

In this argument, it is to be observed that the conclusion against the gravitation theory is arrived at by an unwarrantable assumption of the major proposition of the syllogism. The assumption is, that a dead, or rather a putrid child, is in the same condition, *quoad* gravity, as a living one.

Before proceeding to argue justly, from the fact of frequent malpresentations of dead children, as to the causes of the ordinary presentation of the head, we must take carefully into account not only the new circumstance of death in these cases, but also all other circumstances in pregnancies with dead children, which are or may be statically different from the circumstances of healthy pregnancy. Among these are prematurity, quantity of liquor amnii, the shape of the uterus, and the shape of the fœtus.

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* This argument is strongly stated by Dubois. See *Mém. de l'Acad. Roy. de Méd.* tom. ii. In it he has been followed by many authors. We quote from Dr. Simpson's paper. (*Monthly Journal* for January 1849.)

We know that in the adult the circumstances of death and putridity produce great and rapid changes, *quoad* gravity. The following few observations will aid in determining the effects, in this respect, of death and putridity in the fœtus. The experiments were made in the same way, only on a larger scale, as the pathologist frequently adopts to determine the densities of textures or organs. Having a solution of common salt, of about the ordinary specific gravity of the fœtus, it was easy by adding a little of a concentrated solution to increase, and by adding water to diminish, the specific gravity of the original solution, so as to bring it to correspond to that of the fœtus to be tried. The position assumed when it floated freely in the middle of the fluid is that given in the observations.

Obs. 1. A fresh female fœtus, still-born at the full time, weighing 4 lbs. 12 oz., retained by threads in the fœtal attitude, floated obliquely as to its length, the head being lowest, in a solution of salt of its own specific gravity. In labour, it presented the head.

Obs. 2. A fresh male fœtus, one of twins, still-born at the full time; weight 6 lbs. 14 oz. Floated obliquely, head lowest, in a solution of salt of specific gravity 1050, at 43° Fahr. In labour, it presented the head.

Obs. 3. A fresh female child, one of twins, still-born at the full time; weight 6 lbs. 11 oz. Floated obliquely, head lowest, in a solution of specific gravity 1048, at 43° Fahr. It presented the feet in labour.

Obs. 4. A fresh child, still-born at the full time. Floated nearly vertically, head lowest, in a solution of specific gravity 1050, at 48° Fahr. In labour, it presented the head.

Obs. 5. A fresh child, born alive at the full time, died between the second and third day of life. Floated with point of shoulder a little above surface of fluid of specific gravity 1050, at 54° Fahr. In labour, it presented the head.

Obs. 6. A fresh male child, born alive at the full time, and lived for an hour and a half. Floated like the fœtus in Obs. 5, in a solution of specific gravity 1053, at 54° Fahr.

Obs. 7. A still-born child at full time, had been dead probably for some days before birth. Floated almost horizontally, in a solution of specific gravity 1059, at 53° Fahr.

Obs. 8. A fœtus not far from the full time had died in utero, probably a month before its birth. Bones of head quite loose. Head and abdomen collapsed, as if empty. Floated obliquely, head lowest, in a solution of specific gravity 1070, at 56° Fahr.

Obs. 9. A putrid fœtus, born in seventh month. Abdomen much distended, intestines containing a little gas. Supposed to have died a week before birth. Floated obliquely, head highest, in a solution of specific gravity 1040, at 46° Fahr. In labour, the head was the presenting part.

Obs. 10. A slightly putrid male fœtus of the sixth month; weight 2½ lbs. Floated obliquely, head highest, in a solution of its own specific gravity. In labour, it presented the breech.

Obs. 11. A putrid fœtus of about the sixth month. Head well ossified, and quite collapsed. Floated, head lowest, in a solution of specific gravity 1045, at 50° Fahr. In labour, it presented the head.

Obs. 12. A fœtus born dead, but not putrid, at about the sixth month. Floated obliquely, head lowest, in a solution of specific gravity 1045, at 40° Fahr. In labour, it presented the head.

Obs. 13. A putrid fœtus, of about the fifth month. Floated horizontally, except feet and breech, which were lowest, in a solution of specific gravity 1050, at 38° Fahr. It was born footling.

Obs. 14. A fœtus of about the fourth month, very little putrid. Floated obliquely, head highest, in a solution of specific gravity 1049, at 49° Fahr. In labour, it presented the head. It had been kept till the third day after birth before being experimented on.

For the opportunity of making many of the above experiments, I was indebted to Dr. T. E. Charles, House-surgeon of the Royal Maternity Hospital; and for assistance on many occasions, to my former pupil, Dr. Somerville. The former of these gentlemen made a few observations for me on the density of liquor amnii, which I shall here also give:—*

1. Liquor amnii at the full time, 1008, at 64° Fahr.
2. " " 1011, at 66° Fahr.
3. " " 1010, at 54° Fahr.
4. " " 1005, at 80° Fahr.
5. " " 1009, at 55° Fahr.

The specific gravities of the fœtuses were—

1. Fœtuses at the full time, 1050, 1048, 1055, 1059.
2. Fœtus near the full time, 1070.
3. Fœtus at the seventh month, 1040.
4. Fœtuses at the sixth month, 1045, 1045.
5. Fœtus at the fifth month, 1050.
6. Fœtus at the fourth month, 1049.

Many more experiments like the above will be required before we can use such observations with full confidence. At best, a difficulty will remain, depending on the difference between the specific gravity of the fœtus and of the liquor amnii. The exact position of equilibrium of the fœtus in a fluid, like the liquor amnii, of less than its own density, I am unable to demonstrate. But the above observations may be regarded as giving the nearest easily attainable solution of the problem.

The following conclusions, drawn from these few and imperfect observations, may be stated with considerable confidence.

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* See Observations by Rees.—*Guy's Hospital Reports*, vol. iii.

1. The healthy fœtus floats obliquely, with its head lowest, in a fluid of its own specific gravity—a position corresponding to that which it has in utero.

2. The fœtus has a specific gravity of about 1050, while that of the liquor amnii, at the full time, is nearly 1010.

3. Soon after the death of the fœtus in utero, changes take place in it which alter its position of equilibrium in a fluid of its own specific gravity, so as to be generally the reverse of that of the healthy fœtus; that is, so as to be oblique, with its head highest.

4. It may happen that an advanced stage of decomposition of the fœtus, with collapse of the cranium, may make its position of equilibrium, when floating, again oblique, with the head lowest.

5. These circumstances have probably considerable influence in determining the frequent malpresentations of dead children.

PART II.

THE PELVIS STUDIED WITH A VIEW TO OBSTETRICS.

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CHAPTER I.

ON THE OS SACRUM, CONSIDERED AS FORMING PART OF THE VAULT OF THE PELVIS, AND ON ITS FUNCTION IN THE DEVELOPMENT OF THE LATERAL EXPANSION OF THAT CAVITY.

In introducing my views on this subject to the notice of the profession, I find it necessary to point out the imperfect or erroneous character of the notions which are still largely entertained. This invidious task is recommended by the consideration, that such preliminary discussions will not only tend to dispel error, but will add to the facility of obtaining a clear and accurate idea of the views to be afterwards propounded and defended.

It is surely, now-a-days, a work of supererogation, to say one word in answer to those who would wish to set aside all attempts to introduce into the philosophy of the human frame, the trite laws of mechanical action.*

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* The following pregnant sentences from Rokitansky give a glimpse in few words at this wide field of investigation, in regard to

Our knowledge of physiology, as it now makes daily progress, ever tends to reduce within narrower and still narrower limits the phenomena or functions which are peculiarly designated "vital," a term which is equally useful to screen our ignorance, to excuse our want of zeal and ability in probing nature's secrets, or to indicate our arrival at ultimate laws, beyond which, even the dreams of philosophers cannot anticipate the possibility of reaching.

THE SACRUM AS A WEDGE.

When this bone is looked at in front or behind, its wedge shape is at once evident to every observer. From its base, which has a breadth of 4½ inches, the bone tapers rapidly to its apex or coccygeal extremity. The length of the bone is about 4½ inches, and the anterior borders of its lateral surfaces, which give it the wedge-shape, when produced, approach one another at an acute angle, and almost meet at the apex. When more carefully examined, the bone is found to present a wedge-shape in other points of view than that just mentioned as first apparent. The antero-posterior thickness of the base is about 2½ inches, while that of the apex is only near 2 lines. The bone is therefore wedge-shaped from the

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the pelvis alone ; "It may be regarded," says he, "as an exception to find a completely normal pelvis, when there is any curvation of the spine."—*Manual of Pathological Anatomy.* Sydenham Transl. III. p. 241.

convergence of its anterior and posterior surfaces, as is well seen in the common drawings of an antero-posterior section of the pelvis in a vertical plane. In another and third point of view, the sacrum presents a wedge-shape. We have already ascribed to the convergence of the anterior borders of its lateral surfaces its most evident likeness to a wedge. But these lateral surfaces converge in another direction, namely, upwards and backwards, the bone being broader in front than behind.* Besides these three evidently just comparisons of the shape of the sacrum to a wedge, there is a fourth which a little attention will at once enable any one to apprehend. Unlike the others, it is not constant; and, further, it is carefully to be remembered that in the present question it is not of any importance, unless it be in the direction of the forces influencing the pelvis, or

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* I am satisfied with the general statement made in the text, as it agrees with the ordinary anatomical descriptions of the bone, and tallies with my own observations. But I shall here quote Mr. Wood's account of this subject, premising that the variations from the general statement in the text are either irrelevant to the questions to be discussed, from the measurements not being in lines passing vertically through the sacrum in the erect position of the body, which are the lines of action of the great force, the weight of the body; or else the variations are so trivial in measured extent, as not to produce any very important mechanical action further than what, in machinery, is called a "bite," and even this very imperfectly, from the elastic and yielding properties of the bones and cartilages forming the joints of the pelvis.

"The posterior surface is rough for muscular attachments, and directed upwards and backwards. It is narrower than the opposing parts of the anterior at the upper part of the bone generally, by rather more than half-an-inch. According to Mr. Ward, a transverse section of the sacrum, an inch below the base (at the second sacral vertebra), rather of the great vertical force, the weight of the body. This wedge-shape is found in a section of the bone in a direction, oblique as regards its long axis, and passing from above, that is, from upwards and forwards on the posterior surface of the bone, in a direction downwards, to emerge on the anterior surface, at the site of the lateral notches opposite the second sacral vertebra.

Hitherto I have described the bone as wedge-shaped in various points of view, each of which includes the whole bone or thickness of the bone. But in most, though not in all, sacra, there is an interesting wedge-shaping of the posterior or upper part of the auricular surfaces (see *a b*, figs. 6 and 7). This portion I select for special notice, on account of its importance in several respects, some of which will afterwards appear. The auricular surface is, in most pelves, grooved or furrowed in its whole length.

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shows that in this place the posterior surface is wider than the anterior by three-sixteenths of an inch, so that the sacral wedge is here reversed in obliquity, which he considers of importance in resisting anterior dislocation of the sacrum. Above this point, the anterior surface is three-sixteenths of an inch wider, and below it resumes its superiority in width by four-sixteenths. In some cases, the back and front are of equal width ; in others, the anterior diameter exceeds the posterior throughout."—*Cycl. of Anat. and Phys. Suppl.* p. 118.

Mr. Ward expressly says that the sacrum "is larger above than below, and is also (with certain exceptions which will presently be stated) wider before than behind."—*Human Osteology*, p. 255. The exceptions alluded to are noted above in the extract from Mr. Wood's paper. It is important to remember that these exceptions refer to sections of the pelvis made in the plane of the brim not in a vertical direction.

This furrowing of the auricular surfaces which are parts of the lateral surfaces, and which converge backwards and upwards, is deep enough to give the posterior or upper halves of these surfaces a different direction from that of the auricular surfaces as a whole (see *a c*, Figs. 6 and 7). They are found to converge forwards and downwards (as is easily seen in the figures), and are shaped like a wedge so directed.

Having thus shown how the sacrum resembles a wedge in shape, as far as several distinct arrangements of its surfaces are concerned, it remains to be demonstrated that it does not act in any important respect as a wedge. The unfortunate mental association of the sacrum with this mechanical power should be entirely laid aside, as it can only mislead, if it is not clearly understood that the analogy to a wedge is only in the circumstance of shape, not in its relations as a mechanical element in the construction of the pelvis. This erroneous association has sprung from a most fertile source of error, namely, ignorance of the true inclination of the pelvis in the erect posture, or from anatomists regarding the skeleton of the pelvis as it usually rests on a table, tripod-like, on the coccyx and two ischial tuberosities. When the pelvis, in the living body, is in this position, as in slovenly sitting, then, indeed, the sacrum may act mechanically as a wedge; but its relations in that state to the weight of the body, to shocks and muscular efforts, is comparatively so trivial and unimportant, that we shall not dwell on it here further than to point out how, in this position, the posterior vertical sacro-iliac ligament will come into service in preventing displacement downwards. This ligament is a superficial bundle of the posterior sacro-iliac ligament, whose fibres run in a different direction from those of the great mass of the ligament.

It is in the erect position that the pelvis is subjected to all the mechanical influences which are of paramount importance, in regard to it as a whole, or in regard to its separate parts. The sacrum, as a mechanical element, in this part, must always be studied and considered as in the position it occupies when the body is erect.

Before proceeding further, it will be useful to say that all the posterior or inferior half of the bone may be left out of consideration. It is only the two superior and part of the third sacral bones that are in contact with the ilia, and can have anything to do with the transmission of the weight of the body, or of the paramount mechanical influences to these bones or the rest of the pelvis.

The only parts of the sacrum which can be supposed to convey any wedge-like action to the ilia are, of course, those where contact exists; and it is to these that the study of the wedge-like action of the sacrum must be confined. They are the auricular surfaces, which, on the side of the ilia, as well as of the sacrum, are covered by cartilage, and have a more or less perfect intervening potential synovial cavity. Of these auricular surfaces on either side of the sacrum, the most distant parts (or base of the supposed wedge) are the angles looking forwards and downwards corresponding to the broadest part of the whole sacrum—namely, the lower or anterior border of the superior surface or base. As the other parts of these surfaces recede from these points they approach each other. These auricular surfaces, as a whole, or, in other words, the sacrum as a whole, can act as a wedge, only if the power applied acts in a direction from the broadest part or inferior border of the base. But the weight of the body is a vertical force directed from above and nearly in an opposite direction, and cannot, therefore, have any influence on the sacrum regarded as a wedge. It is, indeed, almost impossible to conceive any force acting in a direction looking towards the broadest part of the sacral wedge. The best of all illustrations of the action of the weight of the body, in a direction nearly exactly the reverse of that required to act upon the sacrum as a wedge, is found in cases of softening of the bones of the pelvis, where the weight of the body always depresses the sacral promontory more or less in a vertical direction.

The weight of the body is transmitted through the sacrum to the strongest parts of the ilia, or iliac beams, which convey it to the heads of the femurs. If, then, we regard the auricular surfaces of the sacrum as seen in vertical sections (see Fig. 6,* *a c*, p. 63), that is in the direction of the force applied to it, we shall discover the shape presented by the sacrum in relation to that force. Such sections pass from the anterior inferior border of the auricular surface vertically upwards, and, in accordance with the description already given of the lateral surfaces of the sacrum, such sections will, as a whole, be wedge-shaped, the base of the wedge looking downwards. This is invariably the case if we regard the auricular surface in totality, but in accordance with the description already given, small parts may occasionally be wedge-shaped in the reverse direction. Further, as already stated, the upper half of the lower or horizontal limbs of the auricular surfaces (see *a b*, Figs. 6, 7), will also be wedge-shaped downwards. Both these last wedge-like arrangements of the sacrum will certainly, when present (which is far from being always the case), tend to act upon the ilia wedge-like under the influence of the weight of the body. But in the former of these two very limited arrangements, the convergence

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* This section is as nearly in a vertical direction through the sacrum, in the erect position of the body, as could be easily attained to. The section is through the second sacral vertebra, nearly in the line

of gravity of the body. The late researches of Meyer have corrected and completed those of Weber, in indicating the position of the centre of gravity as above or in the second sacral vertebra. The centre of gravity may be admitted to be somewhere in an antero-posterior vertical plane. Borelli and the Webers pointed out the horizontal plane in which it lay. The section of these two planes gives the antero-posterior line in which the centre lies. Meyer has completed the investigation by indicating the point in that line where it is.

This section does not pass through the length of the iliac beams, as in Fig. 7; for the iliac beams pass obliquely forwards to the acetabula, nearly in the plane of the pelvic brim; they do not receive the great forces, to which they are subjected in the exact direction of their length.

of the sides of the wedge is very slight, and in the latter, the extent in the vertical direction or length of the wedge is very trivial. These circumstances will annul any marked wedge-like action, and reduce their influence to merely what is called in mechanics, a "bite" or "joggle." Moreover, the descent of the sacrum, so as to act upon the ilia like a disrupting

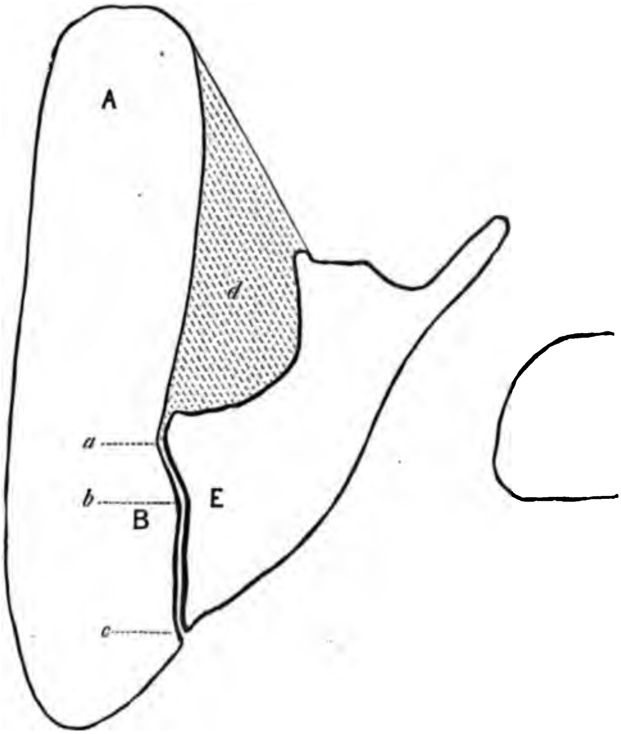

Fig. 6.

Fig. 6. A, vertical section of sacrum and ilium through the sacro-iliac joint. A B is the ilium. E, and the lines to the right, represent the sacrum. *d* is the sacro-iliac ligament.

wedge, is rendered impossible by the massive and powerful posterior sacro-iliac ligament.

Into this subject much mechanical error has been introduced by M. Cruveilhier, in which he has been followed by some of the latest writers on this subject. It is a primal law in physics, that a force transmitted through a solid body is not changed in its direction by any change of the body's direction, nor influenced in direction by the shape of the body. Forgetful of this, these authors, in their zeal to prove the wedge-like action of the sacrum, have erroneously represented the force of the weight of the body transmitted through the spine, as changed in its direction with the changing direction of the different parts of that column, changing also at the promontory of the sacrum so as to be directed downwards and backwards upon that bone.* Under such a supposition the sacrum might be com-

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* "In order," says Cruveilhier, "to understand the advantage arising from this form (the double wedge-shape of the sacrum itself), it is necessary to remark, first, that the weight of the trunk is transmitted in the axis of the upper half of the sacrum, and, consequently, in the direction of a line sloping downwards and backwards; from this it follows, that the sacrum must have a tendency to be displaced either downwards or backwards, but the displacement downwards is prevented by the position of the ossa innominata, which are nearer to each other below than above. The displacement backwards is obviated by the oblique direction of the articular surfaces of the same bones backwards and inwards, whilst the obliquity of the sacrum itself is in the opposite direction, for it is broader in front than behind."—*Descr. Anat.* Engl. Translation, vol. i. p. 204.

"But," says Mr. Wood, "as Cruveilhier has justly observed, forces acting in the curve of the lumbar vertebræ are partly counteracted by the elastic spring-like yielding of the lumbar and sacro-lumbar fibro-cartilages; and by the lumbar curve they are, at the same time pared in action to a wedge. But what I have already said shows such a supposition to be absurd.

The Sacrum as the Keystone of an Arch.

It is unnecessary to spend time in showing that the sacrum cannot act, though often described, as the keystone of the so-called pelvic arch, seeing we have already shown that it does not act like a wedge, a mechanical power to which every keystone must have a resemblance both in form and function.

But even allowing that the sacrum has no function analogous to that of a keystone, there is yet another question to decide, namely, whether the whole arch-like vault of the pelvis, resting on the femora, is analogous to an arch farther than in mere form. Its archlike shape, and the distant similarity of the changes it undergoes in becoming deformed from softening of the bones, to the changes produced by pressure from above on an overburdened arch, make the comparison of the two at least an elegant allusion fitted to impress certain important facts on the memory. But a consideration of the structure of an arch, and of the mechanical advantages which it presents, make it apparent that this comparison of the pelvis is far-fetched.

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directed backwards as well as downwards."—*Cycl. of Anat. and Physiol. Suppl.*, p. 142.

"It is true, indeed," says Mr. Ward, "as Cruveilhier observes, in explanation of his opinion on this point, that the force which the sacrum habitually sustains (namely, the constant pressure of the trunk), tends always to thrust it downward and backward, and never, in any degree, towards the cavity of the pelvis."—*Human Osteology*, p. 261.

The explanation of the mechanical relations of the sacrum which we give in the next section appears to us to be much simpler, and is the only one consistent with the real circumstances of the case.

When the pelvis is described according to this comparison to an arch, as it may justly be, so far as mere shape is concerned, it is divided into two parts. Of these the upper is the larger and stronger, it is subtended by an imaginary line joining the acetabula. The lower is the smaller; it is often called a counter-arch, and is subtended by the same imaginary line as the other. This separation of the pelvic circle into two parts, it is of great importance to keep in mind in studying the development of the pelvis. In the explanations which follow, the anterior arch will be left entirely out of view. The justice of this is not at first sight obvious; but it is evident, if the anatomist recollects that the lower arch is not joined to the upper arch by any other than cartilaginous union, during the time in which the pelvis is becoming developed into its perfect shape—a circumstance which will greatly, if not entirely, destroy its influence in modifying the shape acquired by the upper arch. It is not till after the period of puberty is passed, and after the consolidation of the ischium to the ilium that the pubis at length becomes consolidated with both. This fact, and the slight mobility of the pubic joint, will almost, if not altogether, annul its influence in modifying the development of the upper half of the pelvis.

THE TRUE MECHANICAL RELATIONS OF THE SACRUM.

Having shown that the sacrum is not justly compared to a wedge or to the keystone of an arch, and having, in course of the discussion, pointed out the anatomical peculiarities of the sacrum important in this point of view, we proceed at once to consider the real relations of the sacrum.

The sacrum, then, is to be regarded as a strong transverse beam, curved on its anterior surface, and having its two ends, being the two auricular surfaces, in contact with the corresponding parts of the iliac bones. It is so formed and placed between the two iliac bones, that, under the weight of the body or any vertical pressure, it cannot act as a whole like a wedge. But, as already pointed out, there are certain limited parts of the auricular surfaces of the sacrum so arranged as really to have a wedge-like action. These are so limited, however, as not to be of nearly paramount importance, but act as what are called "bites" in engineering.

The whole weight of all the upper parts of the body is transmitted through the vertebral column to the sacrum. How then is the sacrum retained *in situ?* Above and behind the auricular surfaces are attached the posterior sacro-iliac ligaments [*d*] which unite these parts to the corresponding rugosities on what may be called the posterior iliac tuberosities (see A, Figs. 6 and 7). From the former to the latter parts, the fibres of this, which is the strongest ligament in the body, run obliquely upwards and outwards. The sacrum has nothing to prevent its being depressed under the weight of the body but these ligaments, to which the weight is all transmitted, and again through them to the posterior iliac tuberosities. The whole weight of the body, then, on the sacral beam is suspended by the sacro-iliac ligaments from the posterior iliac tuberosities, which, pro-

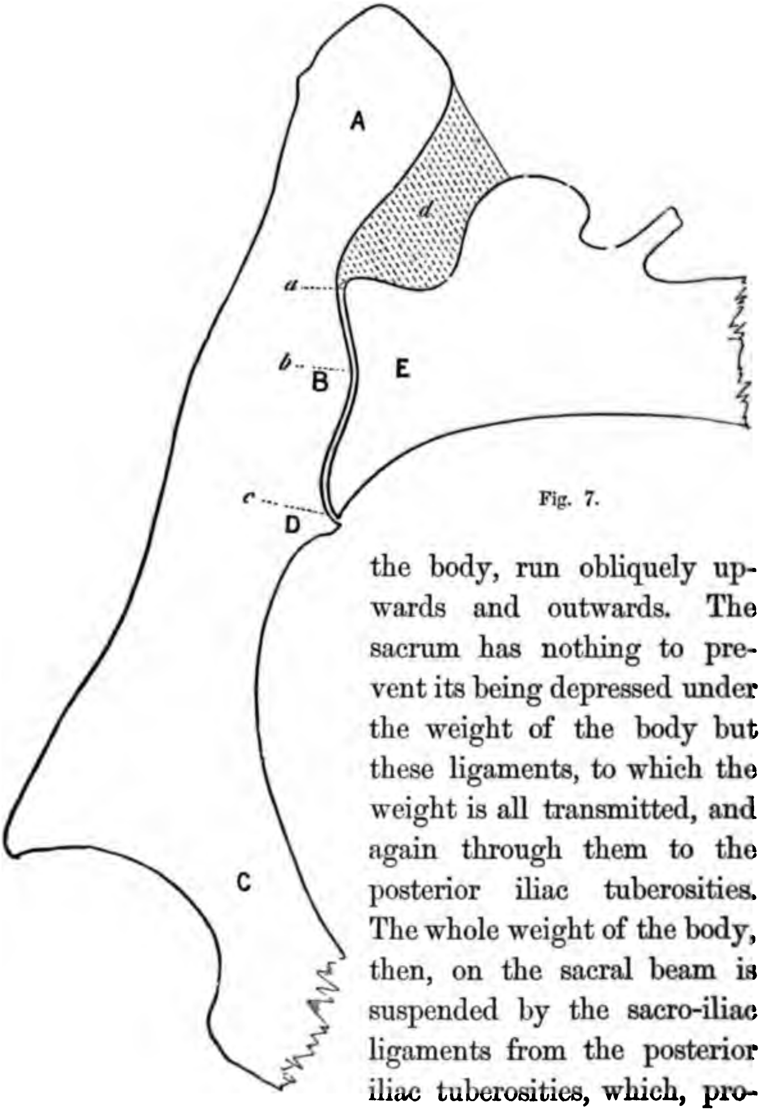

Fig. 7.

Fig. 7 represents a section of the pelvis through the iliac beam, in a plane parallel to that of the brim.

jecting backwards and inwards, overhang the sacrum to some extent.

Once transmitted to the posterior iliac tuberosities, the weight is by them carried to the heads of the femurs through the iliac or cotylo-sacral beams which lie between them. The iliac or cotylo-sacral beam then extends from the posterior iliac tuberosities to the acetabula (Fig. 7, C). This beam is pressed against the sacrum as against a fulcrum (see Fig. 7, B) by forces applied at both its extremities. At the upper extremity, the force is one dragging in a direction corresponding to that of the posterior sacro-iliac ligaments, more or less in a line towards the centre of the pelvic circle. The force at the lower extremity is the reacting force of the weight of the body pushing in a direction upwards and inwards, also towards the centre of the pelvic circle. Both of these forces press the beam upon the fulcrum or auricular surface of sacrum (E), which it is of importance to observe is not in the centre of the beam, but nearer its upper than its lower end. The auricular surface is in mere distance but little nearer the upper than the lower extremity of the beam, but mechanically viewed is much nearer; for while the force applied to the lower end of the beam is applied to its extremity only, that applied to the upper is applied over its whole surface, above and behind the auricular portion, and therefore the length of this arm of the beam is counted, in the mechanical problem before us, only to the centre of the parts to which the force is applied.

MM. DUBOIS AND GAVARRET'S THEORY OF THE CAUSATION OF THE REGULAR DEVELOPMENT OF THE PELVIS.

The only theory of the lateral development of the pelvis, of which I am aware, is afforded by M. Dubois, and is founded upon a study of the changes induced in the shape of the pelvis by the absence of the sacro-iliac joints on one or on both sides. I shall translate into English M. Dubois' own words on the subject.*

The arrangement of the pelvic articulations "has a consequence very worthy of notice and much less known, it is that of favouring the regular evolution of the pelvic cavity.

"When under the influence of a malady, before or after birth, one of the sacro-iliac symphyses is affected with ankylosis, and in consequence the solution of continuity disappears, the innominate bone, corresponding to this alteration, is flattened at the side of the pelvic cavity; the innominate line, in place of offering the normal curvature which I have described, forms a line almost straight from before backwards, which contrasts with the naturally curved form of the innominate line of the opposite side. This kind of alteration, limited to one of the sides of the pelvis, where one only of the sacro-iliac symphyses is ankylosed, is formed symmetrically on both sides when the symphyses on both sides are ossified.

"I think that the ossification of the symphyses, and the consequent disappearance of the solution of continuity in the points where it existed, and where it deadened any shock, take the greatest part in the deformity under discussion, for a similar cause is followed by similar results in other parts of the skeleton; it is thus that the ankylosis of the very slightly movable articulations of the bones of the skull, and, in particular, of one of the squamous sutures, is followed by a marked flattening of the cranial vault in the part corresponding to the union; the Museum of Pathological Anatomy of the Faculty of Medicine of Paris contains a remarkable example of this; and M. Nægele has doubtless alluded to deformities of this kind, after an interesting observation of Tourtual;* my colleague and friend, however, Professor Gavarret, thinks that the regular development of the pelvic cavity results essentially from the wedge-like arrangement of the sacrum interposed between the innominate bones, and that if the lateral regions of the pelvis flatten in some sort under the weight of the body when one of the sacro-iliac symphyses is ankylosed, and *à fortiori*, when they are both so, this effect results principally from the ankylosis making the sacrum lose the form, and, consequently, the mechanical attributes of the wedge.

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* *Traité Complet de l'Art des Accouch.* I. Livr. p. 34.

"I owe to his kindness the following formula, which will clearly explain his idea, as far as regards the physiological action of the sacrum in the normal state of its articulation with the innominate bones, and also the pathological phenomena which result from the contrary state:—

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* *Des Principaux Vices de Conformation du Bassin et Specialement du Rétrécissement Oblique*, par Fr. C. Nægele, traduit par A. C. Danyau, p. 85.

"Suppose a wedge ABC transmitting a force, represented by weight P, to two supports M and N, the force exerted by the weight P is decomposed into two OD and O'D', perpendicular to the surfaces of contact of the wedge and of its supports. In their turn the forces OD and O'D' are each decomposed into two. (OE and OF for OD) (O'E' and O'F' for O'D'). Finally, the total force of the weight P, transmitted by the wedge, is replaced by four forces: 1st, Two of these last forces OF and O'F' are vertical, and form in reality the vertical weight which the supports M and N have to bear; 2d, The remaining two last forces OE and O'E' are horizontal; these last do not in any way affect the supports M and N, but tend only to separate them from one another.

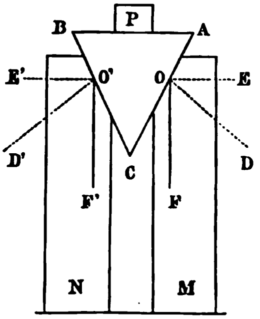

Fig. 8.

"APPLICATION TO THE PELVIS.

"1st, *Normal Condition.*—The sacrum is a true wedge, which transmits to the innominate bones the weight of all the upper part of the body. It follows that the weight supported by the sacrum, at the moment when it is transmitted to the iliac bones through the sacro-iliac symphyses, is decomposed into four forces, as follows:—

"(1.) Two vertical forces, which constitute all the weight which the femora have to support; this weight is necessarily less than that supported by the sacrum itself.

"(2.) Two horizontal forces which do not influence the femurs, but tend only to push the innominate bones from within outwards, to separate them from the median line.

"When, then, in the course of the development of the young subject, the transverse diameter of the pelvis tends to increase, this is what happens:—1. The iliac bones are pressed vertically by the heads of the femurs, and this force resists the enlargement of the pelvis; 2. But the two horizontal forces which the sacrum exerts upon the iliac bones tend to separate them from the median line; it is in virtue of these two forces that the two heads of the femurs are separated from the median line, and the enlargement of the pelvis becomes possible.

"2d, *Pathological Condition.*—Suppose the two sacro-iliac symphyses ankylosed, the sacrum no longer acts as a wedge, the weight of the body is transmitted entirely to the femurs in a vertical direction.

"Then, when the development of the osseous system is being completed, the heads of the femurs will retain the innominate bones in their relative position without there being anything able to counterbalance their vertical force; in consequence, the enlargement of the pelvis may easily happen in an antero-posterior direction, but it will be completely impossible in a transverse direction.

"It is easy to see what would happen if only one sacro-iliac symphysis were ankylosed; in this case, the enlargement of the transverse diameter would be possible only on the side opposite the ankylosed sacro-iliac symphysis. Further, the antero-posterior enlargement taking place as usual, it is evident that the symphysis pubis should be displaced and carried towards the side of the healthy sacro-iliac joint."

This ingenious explanation, by Gavarret, of the mechanical action of the sacrum, and specially of its function in the lateral development of the pelvis, is founded entirely upon the supposed analogy, in form and function, of the sacrum to a wedge. Having already taken some pains to show that the sacrum can have no important action as a wedge, it may be considered a work of supererogation to show that the theory of Gavarret, founded as it is upon this false hypothesis, must itself also be insufficient and useless; but a short discussion of it will be of service. Before touching the subject proper, I shall just notice one very evident mechanical error made in the reasonings of Dubois and Gavarret. This error consists in supposing that the force of a weight acting vertically is not all transmitted downwards, but, when resolved into two forces, is diminished in the vertical direction to an amount equal to that transmitted in directions not vertical. Thus, in the quotation given, it is said that the "two vertical forces which constitute all the weight which the femurs have to support, are necessarily less than that supported by the sacrum," because the weight supported by the sacrum furnishes "two horizontal forces which do not influence the femurs." Again, it is said, in a form converse of the former, that "suppose the two sacro-iliac symphyses ankylosed, the sacrum no longer acts as a wedge, the weight of the body is transmitted entirely to the femurs in a vertical direction." These statements are equivalent to the assertion that the weight of a man's body is not all transmitted to his femurs when the sacro-iliac joints exist, but is so when they are ankylosed, or that a man standing in the scale of a balance would weigh less when his sacro-iliac joints are not ossified than when they are, *cæteris paribus;* which is absurd.*

The following argument, apart from any other, is sufficient to show the erroneous character of the theory of Dubois and Gavarret. That theory is evidently founded upon a study of the circumstances of the oblique pelvis of Nægele. After applying it to the development of the healthy pelvis, they make it explain the oblique pelvis. In this last they point out, that, from the ossification of the sacro-iliac joint on one side, that side loses the developing or horizontal force from the wedge-like sacrum, and therefore remains flat during life. They add, that, on the healthy side, the developing or horizontal pressure of the sacrum produces the normal rounding and expansion of that side of the pelvis. But this case of oblique pelvis, which is at once the source and the test of their theory, does really militate against it, and overthrow it when rightly considered. For it is evident that, when the sacro-iliac joint of one side is ankylosed, the pelvis should be flat on both sides. The ankylosis on one side destroys the wedge-like form and function of the sacrum on both sides. By an incomprehensible neglect, the authors named have argued as if the sacrum were still a wedge, after it was ankylosed to the ilium of one side. When it is so, we find that the iliac bone of the opposite side is almost naturally developed. It cannot be so from any wedge-like action of the sacrum; we must therefore seek for another explanation.*

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* A like error is made by Litzmann, *Formen des Beckens,* S. 20.

Another circumstance shows this wedge theory to be intenible. On the supposition that the sacrum acts as a wedge, we should expect to find the ilia most separated at their points of contact with the sacrum, or where the whole supposed horizontal force of the sacrum is exerted. The fact that this is not the case, appears to me to be fatal to the theory. Every anatomist must know that, in a well-formed pelvis, the brim is larger or more expanded in front of or below the sacro-iliac symphyses than at them. This cannot be the result of wedge-like action of the sacrum, which should tend to produce a contrary result, to which the force applied at the acetabula would also powerfully contribute.

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* See letter of Giraud-Teulon quoted in the Appendix.

Other questionable points in the theory, as stated in the quotation from Dubois, given above, might easily be pointed out; but as their discussion would only tend to confuse my remarks, I shall omit their consideration.

It is only fair to notice, that although the wedge theory of the action of the sacrum is erroneous, yet science is indebted to MM. Dubois and Gavarret for first attracting attention to the sacrum as probably the chief element in the mechanical arrangements for ensuring the natural development of the pelvis. They have also, by their example, directed the attention of physiologists to the study of the oblique pelvis of Nægele, as an invaluable aid in this investigation. It is this study that has revealed to us the importance of the joints of the pelvis. Hitherto they have been chiefly regarded as affording security against fracture of this bony circle, and as assisting to overcome the evil influence of shocks transmitted from below to the spine and brain. The congenital absence or disappearance, during early life, or period of development, of one or other of these joints, as happens in cases of oblique pelvis, is a crucial experiment performed by nature, to lead us to recognise still further the value of these joints, and to point out their presence as an essential element in procuring the natural development of the pelvis.

THE AUTHOR'S ACCOUNT OF THE CAUSATION OF THE REGULAR DEVELOPMENT OF THE PELVIS.

Having in the last section shown that the wedge theory of the action of the sacrum is rather inconsistent with, than explanatory of, the natural development of the pelvis, and that it also fails to account for the production of oblique deformity of the pelvis in those cases where a sacro-iliac joint has been absent during the development of this part of the skeleton, we proceed to state briefly the natural and simple views on this subject, which at once spring from the observation of the mechanical relations of the sacrum, as we have described them. These views will be found to afford an easy explanation also of the oblique deformity of Nægele, a circumstance which reflects no inconsiderable confirmatory evidence upon them.

One of the chief characteristics of the pelvis of the infant, or very young person, is its small transverse dimensions, compared with the antero-posterior, and the flatness of the sides of the cavity. "The parallelism of the lateral as well as the anterior and posterior pelvic walls is, I think, sufficiently marked and general to be considered as a characteristic of the conformation of the infant pelvis, as we shall find it to be of that of most of the lower animals, giving to it a square-sidedness."* Describing the pelvis of the infant at birth, Velpeau says, "its transverse diameters are much shorter than its antero-posterior."* As compared, in these particulars, with the youthful pelvis, that of the adult presents characters quite the reverse. The transverse diameters of the pelvis are then the largest; the sides of the pelvis, instead of being flat, are beautifully curved outwards, the greatest curve being in the iliac or cotylo-sacral beam, near the sacro-iliac articulation. These great changes take place during the growth of the pelvis in youth. They are undoubtedly partly produced by the increased transverse dimensions of the sacrum in the adult,† but only partly, for this will not account for the changes in the shape of the pelvis.

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* Wood. Art. "Pelvis," *Suppl. to Cycl. of Anat. and Phys.*, p. 136.

During the whole period of growth, while the pelvis is increasing in size, and while its joints are still comparatively loose, and the bones not solidified or consolidated, it has ever and anon, in standing, walking, etc., etc., to undergo pressure from two great forces; the one, that of the weight of the body transmitted to the sacrum, and from it to the posterior iliac tuberosities, or upper ends of the iliac beams; the other, the reacting pressure of the same force, transmitted from the femurs to the acetabula, or lower ends of the same iliac beams. Both of these forces tend, as already pointed out, to drag the extremities of these beams in a direction, more or less, towards the centre of the pelvic cavity. This tendency is counteracted by the resistance of the sacrum, which is interposed, like a fulcrum, between the two forces. The power applied to the upper end of the iliac beam (A) will always tend to throw the lower or cotyloid (C) end outwards, and thus to increase the lateral diameters of the pelvis. This tendency is counteracted by the pressure on the other extremity, the cotyloid end of the beam. The natural result, then, will be, that the beam will be bent at its weakest part, that is, near the fulcrum, or auricular surface of sacrum (E). This bending produces the curve of the side of the pelvic cavity. The power applied at the cotyloid extremity of the beam has a leverage advantage over that at the other end, its distance being greater from the sacral fulcrum. Were the iliac beam of uniform dimensions and strength throughout, we might then easily apply the laws of the curvation of beams under forces to the present subject. This is not the case in regard to the iliac beams, for they are thickest and strongest where they have the most direct pressure to endure, namely, at the auricular surfaces or fulcra. It is interesting, however, to notice, that the bending of the lower end of the iliac beam is greatest near the point (D) where it receives the pressure of the sacrum, and decreases towards the lower support of the beam, in the acetabulum; just as happens in experiments on the mechanical properties of beams.

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* *Traité Complet de l'Art des Acc.* Bruxelles, 1835, p. 61.

† For some notes on this subject see Litzmann, *Formen des Beckens*, S. 32, etc.

The application of these views to the causation of oblique deformity of the pelvis is at once apparent. The chief and primary peculiarity of that interesting deformity is the absence, from the earliest periods of life, of the sacro-iliac joint on one side—this is accompanied, in the adult, with flattening of the side of the pelvis, corresponding to the ankylosed joint. This flattening is the main deformity, and results from the ankylosis; the iliac beam no longer existing, and therefore the mechanism, described as ensuring the expansion of the side of the pelvis, being absent. On the other or normal side of the pelvis, the sacro-iliac joint is natural, the iliac beam is in its usual condition, and that side of the pelvis is expanded nearly as usual. When both sacro-iliac joints are ossified, the iliac beams, as described, are absent on both sides, and the transversely-contracted pelvis is the result.

We do not propose at present to point out, as might easily be done, the application of these views to explain other points in this deformity of the pelvis; to explain the production of the horizontal and vertical curvatures of the sacrum, the gradually-diminishing inclination of the brim of the pelvis, and also the mechanism by which the various deformities of these bones are produced. It must not be imagined that every peculiarity of the oblique deformity can be subjected to mechanical solution; of these the atrophy of certain parts of the affected side of the pelvis is one which may perhaps be referred to the great economical law of nature, whereby parts not having their usual functions to perform do not attain their usual development.

There are some interesting suggestions in regard to the oblique deformity, with the mention of which I shall close this chapter.

The simplest method of transmitting the weight of the trunk to the acetabula may be conceived to be by two straight beams diverging from the base of the vertebral column to the heads of the femurs. Such a formation of parts would have been incompatible with some of the functions which nature destined for the pelvis, and especially with the function of parturition through it. To secure the large and rounded dimensions of the pelvic cavity, nature has adopted the arrangements which it has been the object of this paper to point out. An essential element in these arrangements is the presence of the sacro-iliac joints. Were they not present, the large bow-like curve of the side of the pelvis would be always very liable to fracture, from the great forces and sudden shocks often applied to the two ends of the curve (the centre of the sacrum and the acetabulum). This liability to fracture, then, is avoided by the insertion of the two joints. When these joints are absent, nature secures the pelvis against fracture by diminishing or destroying the curvature altogether, and returns, as in oblique pelvis, to the use of the straight, or nearly straight, beams, to convey the weight of the body to the acetabula. The final cause of the peculiar deformity in oblique pelvis is thus rendered evident.*

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* It is my duty here to record my obligations to the late Professor Goodsir for anatomical assistance in this matter; and to Mr. J. Elliott, Lecturer on Natural Philosophy, for many valuable criticisms.

CHAPTER II.

ON THE FORMATION OF THE RICKETTY AND MALACOSTEON PELVES, AFTER THE RESEARCHES OF M. MEYER OF ZURICH.*

IN the production of deformities of the bones, rendered incapable of their natural resistance by rickets or malacosteon, there are two great causes to be considered—namely, the efforts of the muscles attached to the bones, and that part of the weight of the body superimposed on the various bones in the erect position. The sequel to this paper will justify the complete exclusion of the former of these causes from any important place in the explanation of the great deformities—at least, of the characteristic ricketty and malacosteon pelves of the human female. This conclusion might almost be arrived at on *a priori* grounds alone. For, if we compare the irregular action of any muscle, or set of muscles, to the influence of the great and steadily-acting weight of the body, the unimportance of the former will appear on account of its comparative slightness in physical force, its essentially intermitting and temporary character, as well as the antagonism of the various muscles, not to speak of its utter inapplicability, had it any imaginable force or duration, to explain the changes in conformation actually produced. It is, therefore, to the weight of that part of the body above the pelvis, in the erect position, that we direct our chief attention—a weight in regard to the centre of which, its action and its balancing on the pelvis, so many elaborate researches have been made. The weight in question is transmitted through the lumbar vertebræ to the sacrum, and thence, through the iliac bones, to the acetabula.

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* The papers to which the author is chiefly indebted are to be found in Henle and Pfeufer's *Zeitschrift für rationelle Medicin*, N. Folge, iii. Band. 1853. The author would also direct the reader's attention to M. Meyer's articles on the Ossification of Healthy Cartilage, on the same process in Ricketty Bones, on the Erect Position, etc., in several recent volumes of Müller's *Archiv*. (before 1856).

I shall not here enter upon those researches and observations which relate to the more intimate causes of that unnatural weakness of the bones, which is the real source of the deformities under discussion. This unnatural impotence of resistance is produced in two diseases, rickets and malacosteon: the former, a disease of infancy and childhood, common, in its less severe forms at least, and occurring everywhere; the latter a comparatively rare disease of adults, often occurring in connection with pregnancy, and seen in certain unhealthy communities more than in others.

The researches of Meyer go very far to show that these two diseases are, in their essence, the same; that they are both the results of a process analogous to, if not identical with, periostitic inflammation of the affected bones. The cause of the marked difference in the deformity of the rachitic and malacosteon pelves must therefore be sought for in some secondary peculiarity in the affected parts.

In rickets, the still imperfectly developed bones are affected. At the time of the disease coming on, the completed masses of bone are firm, and equal to the amount of resistance required of them; and it is only in cases of long duration of the disease, or of its most severe forms, that these ossified parts lose the firmness and solidity requisite to obviate deformity in them. The masses of cartilage, not yet undergoing the changes preliminary to their becoming ossified, retain also their elasticity and capability of resistance. On the other hand, the masses of spongy cartilage, prepared for ossification, and lying between the already ossified cartilage and that yet unprepared for ossification, are very soft and yielding. In malacosteon, the completely-developed bones of the adult are attacked by the disease in a form analogous only to the very severest cases of rickets, where, as already said, the ossified parts come to be affected. All parts of the bone are simultaneously weakened and made yielding in cases of mollities ossium.

Ricketty bones undergo changes of form chiefly by compression of the spongy cartilage already mentioned, and displacement of the parts connected together thereby. It is only in the severest cases that the solidified bony parts soften and bend. In such cases, the deformity produced may be the same as in malacosteon. Malacosteon produces a uniform and general weakness of the bones, so that the once dense and hardest parts are liable to break: the looser and spongy parts to bend, and, as it were, fold up under pressure. It will be afterwards seen how much the simplicity of the mechanical condition of the bones in malacosteon simplifies the problem of the explanation of the deformity produced.

In limine, it is necessary to define certain points, lines, and planes, which are to be referred to. The pelvis is divided into two half rings or circles, the upper and lower. Of these the former is formed by the sacrum and iliac bones, the latter by the ischia and pubic bones. The sacrum is divided into two parts, the upper or pelvic part separated from the lower or perineal part by a transverse line passing through the middle of the third sacral bone. The plane of the brim of the pelvis is divided into two parts, an upper and a lower, separated from one another by a line joining the two ilio-pectineal eminences.

In comparing the ricketty and malacosteon pelves with one another, and with the normal pelvis, we consider, first—

The Profile Section (Figs. 9, 10, and 11).

In this the following lines form, one with another, a triangle:—1. The conjugate or antero-posterior diameter (*C*). 2. The sacral line (*K*) extending from the apex of the promontory (*p*) to the middle of the third bone of the sacrum (*w*). 3. A line from the last point to the upper part of the symphysis pubis (*s*), called the diagonal direct diameter (*D*). Through

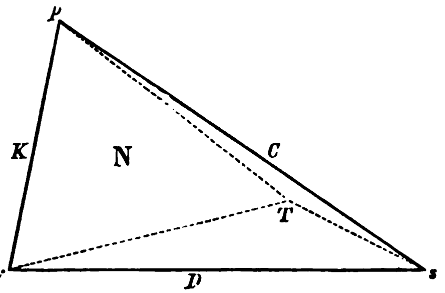

Fig. 9.

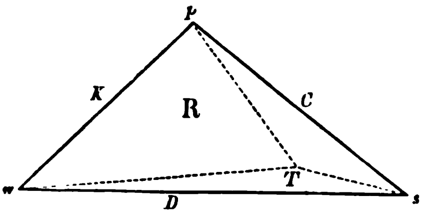

Fig. 10.

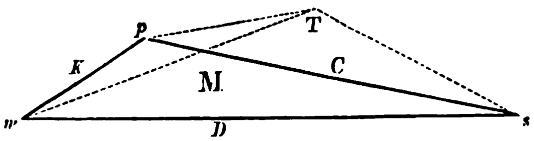

Fig. 11.

the plane of this triangle, at a point *T*, passes a line joining the two ilio-pectineal eminences. From this point lines are drawn to the two angles of the afore-

Fig. 9. N shows the profile section of a natural pelvis; Fig. 10. R that of a ricketty pelvis; and Fig. 11. M that of a malacosteon pelvis.

said triangle at the promontory (*p*) and at the symphysis (*s*). These lines indicate the relative position of the two halves of the plane of the brim. In the normal pelvis these two halves are, as is well known, almost in the same plane.

In three characteristic pelves, the measurements of these lines were as follows:—

		C.	K.	D.
Normal pelvis	N	118 mm.	68 mm.	112 mm.
Ricketty „	R	74 „	66 „	104 „
Malacosteon	M	104 „	38 „	134 „

The distances of *T* from the promontory (*Tp*) and from the symphysis (*Ts*), were as follows:—

	Tp.	*Ts.*
N	78 mm.	40 mm.
R	50 „	28 „
M	49 „	62 „

The distances of *T* from the conjugate *C* were as follows:—

N	3 mm., under the conjugate.
R	11·5 „ „
M	18 mm., above the conjugate.

The distances of the same point *T* from the middle of the third sacral vertebra (*w*) were in all the three pelves nearly the same.

From these measurements it is evident that the two halves of the plane of the brim, which, in the normal pelvis, are almost directly continuous, form with one another, in the ricketty pelvis, an angle open above, and in the malacosteon pelvis an angle open below—in both cases of about 141°.

THE TRANSVERSE SECTION.

In considering this section, we shall regard as forming one continuous whole the two halves of the plane of the brim, whose real relations to one another are described under the profile section.

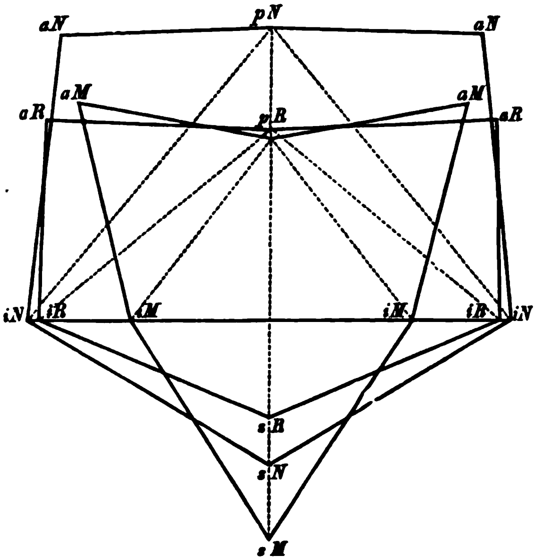

Fig. 12.

Looking at the diagram, we find the promontory (*p*) in the deformed pelvis approximated to the line (*ii*) uniting the ilio-pectineal eminences, almost to the same degree in both. The line itself (*ii*) is in the

In Fig. 12, N stands for Natural Pelvis; R for Ricketty; and M for Malacosteon.

ricketty a little, in the malacosteon a great deal, shorter than in the healthy pelvis. Constructing triangles by the union of the three points, the promontory (*p*), and the ilio-pectineal eminences (*ii*) in each, we find the ricketty resembles the normal base, but has the apex or promontory (*p*) much depressed; whilst, in the malacosteon pelvis, all the three points are approximated to one another. From the observation of these relations (as well as from other sources), it is evident that the causes of distortion act at the three angles of the triangle, and in a direction towards its middle point. In the uniformly-yielding malacosteon pelvis, an uniform diminution of the whole triangle (*pii*, *M*) takes place; whilst, in the rachitic pelvis, there is an approximation of the apex to the base of the triangle (*pii*, *R*), but only a very inconsiderable diminution of the latter.

In the malacosteon pelvis, the sinking of the promontory draws along with it the iliac bone, especially at its upper part, where the spina ilii superior posterior is observed to be flattened, in comparison with the spina ilii inferior attached to the comparatively unmoved third sacral vertebra. Between the part depressed with the sacrum and the acetabulum, the ilium is bent from its natural arc-like form into an acute angle, in which it is followed by the ala and crest of the bone. The depression and folding of the ilia are accompanied by a shortening of the bones by compression, and they form a much more acute angle with the horizon than in the natural pelvis.

It will be observed that, in both the ricketty and malacosteon pelves, the promontory is much depressed, but in very different directions. In the ricketty pelvis, the promontory has descended as if the sacral line (*K*, Fig. 10) had described part of a circle around (*w*) the centre of the third sacral bone, the sacral line (*K*) being unaltered. The ricketty sacrum, as is specially noticeable in the whole ricketty vertebral column, contains points that are prone to yield (the spongy cartilage already described), but besides these it consists in great part of firm ossified portions, and of elastic cartilage, capable of the necessary resistance. The promontory of the ricketty pelvis, then, yields to the pressure, but with the vertical influence of the weight of the body there has to be combined the resistance in the direction of the firm axis of the sacrum; of which two forces, the resultant is the downward and forward movement actually impressed on the promontory. Variations from this course will depend upon the degree of resistance of which the sacrum is capable, and on the degree of fixedness of the centre of the third sacral bone, on which, as a centre, the movement is made.

In the malacosteon pelvis, on the other hand, the promontory (*p*, Fig. 11) has descended almost vertically. The whole bony structures of the pelvis being uniformly, or nearly uniformly, softened, the weight of the body bearing upon the base of the sacrum will tend simply to depress the promontory in a vertical direction, as might *a priori* be expected, and as really happens. In this way, also, the sacral line (*K*) is very much shortened.* The natural pressure of the heads of the thigh-bones upwards and inwards drives the acetabula in that direction. The upward part of the pressure tends to the same end in regard to the brim as the depression of the promontory, and increases the obliquity of the pubic bones to the horizon. The inward part of the pressure has more important results —approximating the acetabula and folding together the pubic bones, so that only a slit-like aperture remains between them. The connection of the acetabula and pubic rami with the ischia causes the approximation of these latter; while the depression of the iliac parts of the bones is naturally accompanied by a pushing forward of the tuberosities of the ischia. In the malacosteon pelvis this is generally well-marked, and is the cause of great deformity of the sacrum. The perineal part of this last bone is strongly connected with the tuberosities of the ischia by ligament, and in the forward movement of the latter parts this part of the sacrum follows, the bone bending about its middle upon the upper or pelvic part, which is differently affected by other forces already described.

In the ricketty pelvis a different series of changes takes place. The projection forward of the promontory is accompanied by a similar movement of the posterior parts of the iliac bones. These bones consequently rest in a more vertical direction upon the heads of the femurs. But as they, at the same time, undergo pressure downwards, their acetabular portions come to be applied more than normally to the posterior parts of the heads of the thigh-bones. The sinking in this direction of the iliac part of the acetabulum drives the ischiatic portion inwards and forwards, as well as the whole ischium, and through the continuity of pressure the pubis will be pushed upwards, and the relation of the two halves of the plane of the pelvic brim, already described, will be produced.

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* The slight deviation of about 7° (shown in the figure) from the perpendicular actually found in Meyer's measurements, he explains by accessory circumstances occurring in individual cases, such as much sitting or lying, etc.

In the characteristic ricketty pelvis, the ilia present none of the folding seen in the malacosteon pelvis. These bones appear to have retained sufficient firmness to be acted upon as levers, in which the power is applied above and behind, the resistance at the acetabula, and the fulcrum at the sacro-iliac articulation between these parts. The yielding and consequent bending inwards of the acetabular portions of the ilia, and change of the direction of the pressure upwards thereon, favours this divarication, or at least greatly diminishes the natural influence of the upward and inward pressure of the thigh-bones in diminishing the same divarication.

In this very sketchy chapter I merely indicate the general direction and results of the researches of M. Meyer and others on this subject. They will be found to be extremely valuable, in affording us not only an easy and satisfactory explanation of the mode of formation of these extraordinary deformities, but also a very easy method of describing to others, and remembering ourselves, the actual form of the parts of what at first looks as indescribable as an Indian puzzle. Besides this, they have most important bearings on the theory of the erect position, and other subjects of high importance in physiology.

CHAPTER III.

ON THE DEVELOPMENT OF THE FEMALE PELVIS.

FOUNDING on the principles established in preceding chapters, and which have, since they were originally published (see Appendix), received ample confirmation by authors in this and other countries,* I engaged in the study of the deformities of the pelvis. Some of the results of the investigation, having reference to the oblique-ovate pelvis, have already been briefly given (p. 81). My natural progress to the rachitic and malacosteon deformities of this part was interrupted by the paper of M. Meyer on this subject coming under my notice. In this memoir most of my views were more than anticipated, and I have satisfied myself with embodying some of the results of his investigations in the preceding chapter.

Considerable discussion has taken place regarding some of the questions raised in these papers. But the views expressed regarding the mechanical conditions of the sacrum have been so generally accepted, that I will not return to their support, but will now assume them as established. The application of these views to the explanation of the development of the pelvis in healthy and morbid states, has been in some quarters received with hesitation, or altogether opposed.* Much of this, I believe, has been owing to the brevity and imperfection of my first descriptions and arguments; and having now reconsidered the matter, with the aid of the criticisms alluded to, I am desirous to confirm my former statements by an exposition of more matured investigation of the subject. I shall hold it proved that the sacrum is neither a wedge nor the key-stone of an arch, and confine myself to the development of the lateral expansion of the pelvis. I shall call to my assistance the anatomy of deformed pelves, believing that they may be regarded, for the purposes of this inquiry, as experiments performed by nature to aid in the discovery of the parts played in the development of this osseous circle by its component bones and joints.

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* See Meyer, *Lehrbuch der Physiologischen Anatomie;* Giraud-Teulon, *Mécanique Animale;* Hubert and Valerius, *Mémoires de l'Académie Royale de Médecine de Belgique*, tome iv.; and *Gazette Médicale de Paris*, p. 722, 1856.

It is necessary to premise that certain average conditions and measurements of the pelvis are assumed as the natural ones; but in no case will it be found that any novelty or difference from generally-received statements is here introduced. Further, all minor influences affecting the mechanics of the pelvis are thrown aside out of consideration; and this for two reasons—namely, that they are unimportant when compared with the great forces and important principal parts considered; that is, that though undoubtedly useful and influential in their own spheres, their influence on the great questions to be now discussed must be so inconsiderable that they may be neglected in a young investigation, the great elements of which are first to be settled. In addition, they would, if considered, be found to introduce so many difficulties and complications as to remove the subject almost, if not altogether, from the field of investigation. Among this class of points are the relations and positions of single muscles, ligaments, or the various parts of bones.

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* See Moore, "On the Sacrum," *British and Foreign Medico-Chirurgical Review*, vol. xx. 1857; also Wood, "On a specimen of Pelvis Oblique-Ovata," *Transactions of the Pathological Society of London*, vol. vii. p. 292.

In observing the influence of the mechanical conditions of the iliac beams on the development of the contour of the pelvis, it is of course necessary to confine our attention to the ilia while undergoing growth and change, and before they have become firmly solidified into their permanent shape. This period extends from childhood, or from the commencement of walking to the age of about twenty years. In these early years the os innominatum is divided into three parts—the ilium, ischium, and pubes—all connected by cartilage. The sites of connection are the acetabula and the sides of the pubic arch. In the latter positions, ossification and consolidation are said to take place about the age of seven years; but in the former this is not effected till the age of about twenty years. It may therefore be held that, till the age of twenty years, the pelvis may be, to a great extent, moulded by the strong forces to which it is subjected, or that its mechanical conditions, as they are less stable, are more influential and important than afterwards.

During early life, the bones are more susceptible of physical influences than afterwards; for they are more elastic and flexible, and are believed to contain less earthy matter than in adult age.*

The pelvis, during its growth, is subjected, in the manner to be afterwards described, to very powerful forces, and, at the same time, it also undergoes considerable changes of shape. It will be shown that these changes of shape correspond to the forces in every ascertainable respect. But before proceeding to show the relation of the forces to the shape produced, it is necessary to say a few words by way of illustrating how mechanical forces affect the shape of healthy bones.

It is surely indisputable that the healthy growing pelvis, as a whole, with its joints and with the Y-shaped cartilages separating the cotyloid ends of the three parts of each os innominatum, is greatly under the control of the great forces to which it is for long periods subjected; and this admission is sufficient ground for the explanation of its development to be afterwards given. A somewhat apposite illustration of this kind of change of form is found in the spinal column, which in the young child is almost straight, but, under the influence of the weight to which it is subjected, assumes the curves natural to it in adult age.

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* Regarding the proportions of earthy to animal matter in bones of different ages, chemists seem not to be quite agreed. "It is usually stated," says Dr. Humphry (*On the Human Skeleton*, Cambridge, 1858, p. 5), "on the authority of Davy, Schreger, and others, that there is a progressive and considerable increase in the earthy constituents of the bones with advancing years. This, however, would seem to be by no means universally true. Dr. Rees finds it to be the case with regard to the long bones and the bones of the head; those of the fœtus not containing the excess of earthy matter which he discovers in those of the adult. But the bones of the trunk in the fœtal skeleton appear, from his analyses, to be as rich in the proportion of earthy matter as those of the adult." It may be worth while, as throwing some light on this subject, to call attention to the posthumous alterations of the shape of bones produced by tomb-pressure and probably under peculiar chemical changes. See the papers of Professor Wyville Thomson on Distorted Human Skulls, *Natural History Review*, October 1862; and of Dr. Johnson of Shrewsbury, *Proc. Roy. Soc. London*, 1862.

But it is quite consistent with our general knowledge of the physiology of bone, that, under powerful and continued forces, even the healthy bones themselves undergo changes of form.* The human skeleton is so arranged as to have, in the adult at least, sufficient power to resist almost every tendency to change of form from this cause. But even in the adult the weight of the body acting at an extraordinary advantage on the neck of the femur, effects in it a slow change of shape, so that it is virtually bent upon the shaft, gradually forming with it a less and less obtuse angle; and from this and other causes combined, it becomes more and more liable to fracture.

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* That this pressure is without influence in the healthy skeleton appears to be the view expressed by Mr. Moore, in a valuable paper on this subject, published in the *British and Foreign Medico-Chirurgical Review* for July 1857. "But," he says, "it is surely erroneous to say that bones depend upon pressure for their regular development. Undue pressure may deform them, but not that they are made to resist: had they no power of growing into a definite shape and maintaining it, they would be incapable of bearing the very forces to which they are ordinarily subject." The study of the contour of the pelvis, varying as it does with every distortion of the spine, seems alone sufficient to modify greatly the view Mr. Moore appears to hold.

The iliac beam may be regarded as a long bone, strengthened by the apposition of its ala. The weight or force to which it is subjected acts upon it at its two extremities, and not in the direction of its length, and with a degree of vantage not to be observed in any other bone of the body. It does, while growing, change in form under these forces, and, I believe, in consequence of them. A like change occurs in other long bones similarly treated, as in the femora of young lads who ride much on horseback.[*] Moreover, when the iliac beams are inordinately subjected to the natural forces in youth, their bending increases to an abnormal extent, as happens in factory children who have never suffered from rachitis, and whose skeletons present no evidence of the previous existence of the disease.[†]

In the field of pathology many illustrations occur, under the most varied circumstances, of the great influence of long-continued though slight mechanical forces in changing the shape of healthy bones.[‡]

It is indeed inconceivable that elastic living tissues should endure powerful and continued pressure without being greatly and permanently affected by it. "The shape," says Dr. Humphry,* "of the bones (and here it may be remarked that the uniformity with which they acquire their proper shape is truly marvellous) must be due chiefly to those same developmental forces whereby the shape of the body generally is determined. Future investigation may point out the proximate causes by which shape is evolved: at present we have little or no clue to them. There are, however, some few secondary agents—assistants, as they may be termed, to the primary developmental processes—whose influence we can trace in moulding the shape, etc., of the bones: one of the chief of these is pressure. The effect of its operation may be observed in a variety of ways. Thus the modelling of the cranial, thoracic, and other bones, upon the parts enveloped by them, must be, in some measure at least, a result of the pressure of those parts upon the osseous material in its soft growing state. The furrows and channels of the bones are, in like manner, partly originated and preserved by the pressure of the tendons, nerves, and vessels upon them. They do not exist when these are

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[*] For a variety of facts illustrative of this subject, see Soemmerring, *Traité d'Ostéologie* (French translation by Jourdan, p. 23). See also Livingstone's *Zambesi and its Tributaries*, p. 116.

[†] The converse of this appears to be true. Hubert (*loc. cit.*) makes the following statement :—"Ainsi chez une naine âgée de vingt et un ans, idiote, bien constituée d'ailleurs, mais qui n'avait jamais pu marcher, Nægele a trouvé le bassin semblable, pour la forme et la grandeur, à celui d'un enfant de sept à huit ans."

[‡] See Rokitansky, *Pathological Anatomy* (Sydenham translation, vol.

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iii. *passim*), for a description of the effects of distortion of the spine on the shape of the pelvis. See also the more elaborate description by Breisky of the uniform result of kyphosis when it has, under certain conditions, influenced the development of the pelvis—*Ueber den Einfluss der Kyphose auf die Beckengestalt* (Separat-abdruck aus den medizinischen Jahrbüchern, i. Heft: Iahrgang, 1865). Some peculiar specimens described by Brandau [*Beitrag zur lehre vom allgemein oder gleichmässig zu engen Becken:* Marburg, 1866] seem to show the great influence of other and more recondite powers than pressure on the development of the pelvis.

* *Treatise on the Skeleton:* Cambridge, 1858, p. 48.

absent, and they disappear, becoming filled up, when these are removed. The curvatures of the bones, though chiefly attributable to the forces of development, may also, in some slight degree, be owing to the influence of pressure—some to the pressure of adjacent organs, some to pressure caused by the action of the muscles pulling upon them in the fœtal state, or by the weight of the body compressing them afterwards. It has been before remarked, and may be again mentioned in connection with this subject, that the bones are, as a general rule, most curved in those persons whose muscular strength is greatest—that is to say, when the pressure upon them resulting from muscular action is greatest; weak persons, on the contrary, provided they be not ricketty, have, for the most part, comparatively straight bones."*

During their growth—that is, from early childhood up till about the age of twenty—the iliac beams are subjected to two great forces. These forces are susceptible of most advantageous easy study, if we assume the body to be standing in the erect position. One of these forces is exerted by means of the superior or posterior sacro-iliac ligament passing upwards and outwards from the posterior rough portion of the lateral articulating surface of the sacrum to the corresponding portion of the ilium between its auricular surface and the posterior processes. The other force is applied through the head of the femur to the acetabulum, and (confining our attention to the ilium) to that part of the iliac bone contributing to the formation of the hip-joint.

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* Professor Turner has referred me to Herbert Spencer's *Principles of Biology*, for some valuable remarks on the shapes of vertebrate skeletons, in the morphological part of his work.

During the first twenty years of life the acetabular extremity of the ilium or iliac beam is joined to the neighbouring portions of the pubis and ischium by cartilage only, and therefore forces acting upon it may be more safely regarded separately, or as distinct from their action on the rest of the acetabulum, than if the cotyloid cavity were a solid cup of bone. Cartilaginous union even must be admitted to introduce new modifying forces for consideration; but even were the acetabulum formed of solid bone, the change would not essentially alter the direction or action of the force affecting the pelvis through the femora,—a point which will be more evident as we advance, and especially when the evidence afforded by observation of the malacosteon pelvis is properly considered.

The two great forces are opposite to one another, and equal to one another, as are action and reaction. When regarded as pressures, or devoid of momentum, they are severally equal to the weight of the half of all those parts of the body above them, if the body be supported on two limbs. But if the body be supported on one limb, as is for brief periods at least often the case, then the forces are doubled; and to this the weight of the limb not supporting the body is also added. But the forces are subjected to great variations on the side of increase if clothes and burdens are taken into account. These, however, are inconsiderable, compared with the great and sudden augmentations when momentum is given to the weights producing the forces, as happens in jumping, and, to a certain extent also, in running and walking. From these considerations, it is apparent that the forces are in long-continued daily action, always considerable, and often very powerful.

The increase of the forces when momentum is communicated to the body, as in walking or running, must be of great importance from its amount and the frequency of its application. But the great and sudden increments produced by shocks, as in jumping, falling on the feet, and such like, however important in other respects, can have no great influence on the development of the contour of the pelvis. Such shocks are sometimes the cause of fracture. From this accident the pelvic circle is often saved by the insertion of three joints in it, and especially by the sacro-iliac joints, where all the ordinary arrangements for the diminution of the effects of a blow or shock are in play. In practice, however, this bony circle is found to be amply protected, for it is seldom broken except by direct violence applied to it. Nature has warded off danger to its integrity by placing the neck of the femur in the line of the shocks or blows from which it would otherwise run most risk. This neck of bone is generally a weaker part, and frequently the weakest part subjected to risk of fractures, and therefore the first to yield,—a circumstance which prevents the far more dangerous fracture of the pelvis.

The direction of the chief force is, like that of all weights, vertical. It is transmitted from the sacrum to the posterior parts of each iliac beam, as already said, through the strong superior posterior sacro-iliac ligament. As this beam is not vertical, but directed downwards and outwards, and as in its course it is supported on the auricular facet of the sacrum as upon a fulcrum, it is evident that the force acting upon its upper portion will tend to draw it inwards, and therefore to throw the lower or acetabular end outwards. That the weight of the body acts in a vertical direction on the sacrum requires no proof. Its effects are seen in the vertical depression which this part suffers in softening of the pelvis. Equally self-evident is the direction of the force acting on the upper part of the iliac beam, and any one may find this also confirmed by observation of the deformities of the pelvis. To produce equilibrium, the reacting forces must be equal and contrary. They are applied to the lower end of the iliac beam and the cotyloid cavity. In order to resist the vertical force downwards, there must be a corresponding reactionary force upwards; and, in order to balance the force throwing outwards the lower end of the beam, there must be a force inwards. The combined forces will act in a direction upwards and inwards. The upward force is easily accounted for: it is the supporting of the weight of the body. The inward force cannot be supplied by the pubic bones; these can only act to some extent as a tie-beam to fix the lower ends of the iliac beams. The direction of the conjoined forces is indicated by that of the neck of the thigh-bone, and the inward force is supplied by the enormous muscles and some of the ligaments which are in action in the erect position, and this with such force as to keep the head of the thigh-bone securely in the cotyloid cavity,—a result quite impossible without very powerful inward pressure. If further proof be demanded of the upward and inward direction of the force applied to the lower end of the iliac beam, it is easily found by a reference to the malacosteon and ricketty pelves. Another illustration we propose to give in a separate discussion of the oblique-ovate pelvis. In the malacosteon pelvis the direct upward and inward direction of the movement which the acetabulum has undergone is proof of the force's direction. In the ricketty pelvis the acetabulum is also displaced upwards and inwards as far as the iliac beam is concerned, although other peculiarities in the history of the production of the deformity, especially the anatomical conditions of early life, modify the general direction given to the acetabula by the pressure of the femora. These peculiarities it would be out of place here to enter upon.

That the lower ends of the ilia must have, during their growth, this tendency outwards, is shown also by the fact, that they are not forced inwards by the reacting force exerted on them through the femora. When the healthy mechanism of the iliac beam is perverted by destruction of the sacro-iliac joint, and the tendency outwards of its lower end is thus annihilated, then the long-continued powerful pressure upon the acetabulum is unresisted by any counteracting force, except the elasticity of the bone: it pushes it inwards, and the result is the production of the pelvis of Nægele if the joint does not exist on one side only, and of the pelvis of Robert if the joint is absent on both sides. It is likewise observed, that increase of weight applied on one side of the pelvis is accompanied by a corresponding, and then morbid, because unsymmetrical, increase of effect on the side most powerfully acted on. The evidence in proof of this last statement may be drawn from various sources. Meantime, it is sufficient to refer to Rokitansky's observations, above cited, and the case of Wilkie in the Memoir of Hubert, and others.

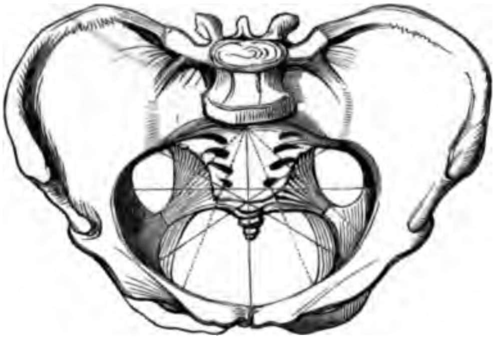

Fig. 13.

Between infancy and adult age the pelvis not only grows in size, but gradually undergoes considerable changes in shape. In early life the antero-posterior

Fig. 13. Pelvis of adult female, after Dubois.

diameter of the brim is greater than the transverse. In the adult, the opposite is the case.* In early life the sacrum is much more nearly vertical in the direction of its length than in the adult. In early life the ilium is nearly a flat bone, and almost straight from the posterior tuberosities to the acetabulum—that is, in the line of the iliac beam. This may be regarded as its spontaneously-assumed developmental shape. In the well-formed adult female the comparison of the iliac beam with that of early life is complicated by the thickening which takes place at or opposite the auricular surfaces, and near the acetabulum; but, disregarding these, it will be observed

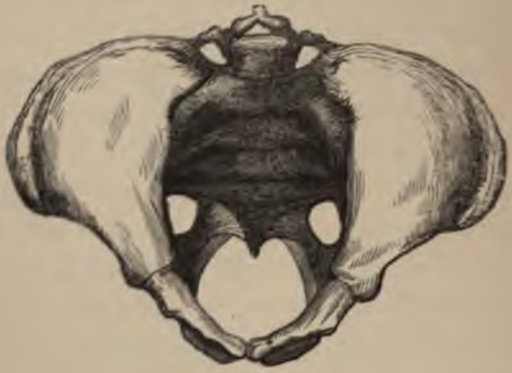

Fig. 14.

Fig. 14. Pelvis of a child.

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* Litzmann's statement is, "Noch bis zum, 13ter Lebensjahre wird indess der Querdurchmesser der Eingangs bisweilen kleiner gefunden, als die conjugata vera, nach dieser zeit constant grösser.—*Die Formen des Beckens:* 1861, S. 33.

to be curved like the segment of a large arc. This curvature is the result of the direction inwards and upwards of the articulating surface, above and behind its auricular portion, and of the direction inwards and downwards of the part adjoining the acetabulum. The curvature* is not exactly circular, but is greatest at the thinnest or smallest part of the beam—namely, a little below the sacro-iliac joint, and a little above the acetabulum. This corresponds to the extremity of the greatest bis-iliac or transverse diameter of the brim of the pelvis. In early life the horizontal rami of the pubic bones form with one another a much more acute angle than in the adult. In the latter the increased distance of the acetabula from one another is followed by a reduction of the anterior projection of the symphysis pubis; the horizontal rami of the pubic bones then pointing to one another at a very obtuse angle.

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* In his article already cited Mr. Moore appears to deny the occurrence of this change altogether. His words are—"Moreover, Dr. Duncan's explanation is misapplied. The cotylo-sacral beams are not curved at all. From their extremities at the tuberosities of the ilium to the acetabulum, although not vertical, they are straight. They have an appearance of curvature from their forming part of the curved brim of the pelvis ; but, in fact, they spread out below into the higher part of the broad acetabula, and acquire, as they spread, a concavity on their external as well as on their internal side—not a convexity, as would be the case if they were curved in the manner described by Dr. Duncan." The error of Mr. Moore's description will be evident to any one who compares the flat ilium, and its nearly straight iliac beam, with the same bone in the adult female, forming an important part of the circle of the brim. The same may be demonstrated by comparing the bis-iliac or largest transverse diameter of the pelvis with the diameter posteriorly between the sacro-iliac joints, and anteriorly between the acetabula. It must be admitted that, in many coarse masculine pelves, approaching closely as they do to the infantile type, the curvature of the iliac beams is scarcely appreciable ; but my whole object is to explain and account for the large and highly-developed female pelvis.

Such are the changes occurring in the pelvis during its growth. They are most truly epitomised in the statement that the transverse diameter of the brim is greatly increased at the expense of the antero-posterior or conjugate. This transverse enlargement is powerfully resisted by the pressure upon the acetabula. In spite of this, it takes place, and the important question arises—What is the mechanism which not only balances the lateral pressure on the acetabula, but overcomes it, separating them further and further from one another, and which moulds the female pelvis into its characteristic shape?

It must be evident to every one, that, as M. Hubert has well shown,* the increase of transverse diameter of the sacrum and the increase of length of the innominate bones, points in their natural developmental progress, have an absolutely essential place in the production of these changes, in so far as without them the pelvis would necessarily be much deformed. Mr. Wood † has described these same changes in the sacrum and ossa innominata, with some anatomical details regarding their osseous centres; but he goes further, and seems to think that the shape of the pelvis is regulated by these circumstances, and that the mechanical conditions I have described have little or no influence. It appears to me, however, as already said, that the powerful mechanical influences on the pelvis cannot be without importance, and that a decided change of shape is observed to take place in it, which is not accounted for by reference to natural development, but receives an easy explanation if we keep in view the physical forces referred to.

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* *Loc. cit.* p. 21.

† *Transactions of Pathological Society*, 1856, vol. vii. p. 296.

Having already (see page 78) fully described the power, direction, and peculiarities of the forces to which the pelvis is subjected, and the conditions of the pelvic bones themselves, as well as shown that these forces cannot be without great influence, very few words will be required to show how their study explains to a very great extent the gradual change undergone by this part between childhood and adult age. The weight on the upper end of the iliac beam tends to draw it inwards, the auricular portion of the sacrum being the fulcrum on which the beam rests. In proportion as the upper end of this beam is dragged inwards, will the lower end tend to be projected outwards, and the transverse diameter of the pelvis be increased. In a well-formed pelvis, with large transverse diameter, this end of the beam will be observed to be more drawn inwards or flattened downwards than in a masculine pelvis, or one without a large transverse measurement. The extravagant enlargement of the transverse diameter of the pelvis is prevented by the pubic bones and symphysis joining the lower extremities of the two iliac bones together. But a study of the ricketty and malacosteon pelves will show how little influence these parts have, when not firmly ossified, in preventing the abnormal separation or approximation of the lower ends of the ilia. When the ossa innominata are completely ossified and healthy, then the influence of the pubic bones, as a tie to connect and as a beam to keep separate, must be such as to render none else necessary. But before the ossa innominata are completely ossified, the great force preventing the extravagant transverse enlargement of the pelvis is the inward and upward pressure on the acetabula. Between these forces, applied to the upper and lower ends of the iliac beams, is the auricular surface of the sacrum, acting as a fixed point upon which the two forces tend to bend the bone. The bone does slightly bend about midway between its auricular surface and its lower end. But although it bends under the influence of these forces, its lower half is, as a whole, to some extent projected outwards. This bending and projection outwards form the characteristic changes undergone by the pelvis during its growth, and give the female pelvis its peculiar characters. In an elegant female pelvis these changes are best seen, for in it the bones are generally more delicate, and the iliac beam is longer than in the male. In a masculine pelvis the changes are less marked, for in it the bones are thicker, and stronger, and shorter, and earlier consolidated with one another. These conditions are at once the signs and the causes of the peculiarities of a masculine pelvis.

Further and irresistible evidence of this being a true explanation of at least a great part of the development of the pelvis, will be found in a study of the changes undergone by this part in rachitis and malacosteon, and in ossification of one or both sacro-iliac joints.

ILLUSTRATION AND CORROBORATION OF THE PRINCIPLES ALREADY LAID DOWN, DERIVED FROM THE STUDY OF THE PELVES OF NÆGELE AND OF ROBERT.

Before entering on this subject, I shall describe a characteristic specimen of the oblique pelvis of Nægele that is contained in my museum, and of which no account has hitherto, so far as I am aware, been published. It is only a fragment of an entire pelvis; the right ilium having been disarticulated, and the lower bones of the sacrum broken off and lost with it. The bones are well developed, and in this respect have nearly as much resemblance to those of a male as to those of a female.

There is no sacro-iliac joint on the left side, nor any trace of it. But its usual locality is to be detected at several parts, and the limits of the sacrum and ilium satisfactorily made out, by connecting them at those places, such as the brim of the pelvis, offering no indications. The union of the two bones is marked at some points merely by the different arrangement and direction of the lines on the surface; at others it is denoted by a very insignificant smooth ridge. Not only is the ossification complete at the usual site of the auricular surfaces, but behind this the posterior sacro-iliac ligament is replaced by a mass of bone continuous with the neighbouring parts, and undistinguishable from them; a point which probably has important bearings upon the origin of this malformation. The left ilium is flattened and pushed inwards, and consequently the symphysis pubis is displaced to the right side. A line at right angles to the transverse diameter of the sacrum, from the right side of its superior articulating surface, falls on the symphysis pubis. The sacro-pubic diameter is 3½ inches; the diameter measured from the promontory of the sacrum to directly above the foramen thyroideum is 2½ inches; the sacro-cotyloid diameter is 2 inches; the distance of the sacro-iliac symphysis from the symphysis pubis is 4½ inches; the right oblique diameter 4 inches. The breadth of the base of the sacrum is 4 inches, and, dividing it at the anatomical centre, the breadth of the right half is 2¼ inches, of the left 1¾ inches.

A great variety of opinions and arguments has been brought forward regarding the cause of the peculiar deformity of the oblique-ovate pelvis. Most of these discussions, regarded philosophically, must be considered as remote from the subject. They refer to questions which, if settled, leave the real explanation of the matter unperformed. For instance, let us suppose it to be concluded that the sacro-iliac anchylosis is the chief cause (a concluison which, in the sequel, I wish to support); it is necessary, before adopting this conclusion, to show how the effect results.

This last part of the demonstration has been sketched only by a few recent authors. Without such a demonstration, the arguments regarding the cause of the deformity go no further than showing how some single point, imagined to be the essential one, is produced; as, for example, that the anchylosis is the result of intra-uterine disease, or that it is the result of abnormality, or absence of certain centres of ossification, etc.

The opinion of Nægele, that oblique deformity of the pelvis, and especially the anchylosis of the sacro-iliac joint, are not results of disease, but of abnormal development, has received the support of so many observations, and of so many distinguished authors,* that I am content to regard it, for the present at least, as determined. In the argument which follows, this class of questions is scarcely relevant.

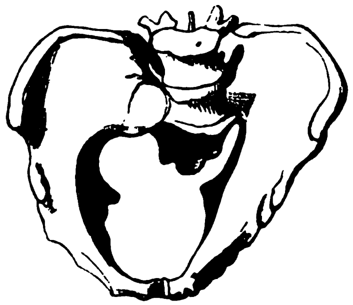

Fig. 15.

It is here assumed that, before childhood commences,

Fig. 15. Nægele's or the oblique-ovate pelvis.

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* See Braun : *Lehrbuch der Geburtshülfe*, p. 424 ; and the discussion of this point by Grenser in a subsequent chapter of this work.

or not later than this time, the sacrum and ilium on one side are not separated by an articulation, and that the bones, especially the half of the sacrum on the affected side, do not grow in equal degrees with those of the normally-developed side.

During growth, the whole pelvis is subjected to great forces, under the influence of which it moulds itself into its characteristic shape. Now, the question of causation resolves itself into this:—Why does the pelvis deviate from the ordinary shape, and assume the oblique deformity, when it grows and is moulded, not under normal conditions, but under the abnormal conditions of anchylosis of the sacro-iliac joint on one side, and atrophy of the parts of the bones joined? And the same question may be asked regarding the transversely-contracted pelvis of Robert; the abnormal conditions in it being present on both sides of the pelvis, instead of on one side only, as in the pelvis of Nægele.

It is scarcely necessary to say that the atrophy of one side of the sacrum, and the less considerable atrophy of the corresponding os innominatum, form important points in the general form of an oblique pelvis. But it appears to me that the most important particular in the deformity of this pelvis—namely, its peculiar shape—has in most cases very little dependence on the atrophy. The occurrence of cases of oblique deformity without any atrophy, and with or without anchylosis of the sacro-iliac joint, is sufficient to show that atrophy is not essential to its production. Further, the abnormity of shape, or rather the pushing in of the side of the pelvis, its chief characteristic, is not in proportion to the atrophy; those pelves having the affected side of the sacrum very little less than the other being as characteristically deformed as those with the greatest atrophy. It is to be noted that, in speaking here of characteristic deformity, I refer only to the bones of the affected side, not to the degree of diminution of the pelvic passage. This last will necessarily bear a direct relation to the atrophy of the sacrum. The deformity may be equally characteristic under different degrees of atrophy of the sacrum. The gravity of the deformity in practical midwifery is a different question, and is in proportion to the degree of diminution of the pelvic diameters.*

It is indeed more natural to suppose that the atrophy of the sacrum would tend to diminish the deformity of the os innominatum or increase the lateral bulging or expansion of the pelvis, inasmuch as the distance between the atrophied side of the sacrum and the symphysis pubis is less than the distance between the side of the fully-developed sacrum and the same part. In other words, an os innominatum of natural length should form a greater lateral expansion than natural when the side of the sacrum to which it is attached is atrophied. In some cases, the os innominatum of the affected side has a length equal to that of the other side, but oftener it is shorter. This shortness must be of great importance in increasing the flattening. But that this shortening or atrophy of the os innominatum is not a chief cause of the deformity, must be conceded if we regard the circumstances that in some cases it is not shortened, and that, instead of being deficient in length to reach the pubic portion of its neighbour, the symphysis pubis is generally displaced by the pubic end of the affected bone towards the healthy side. Moreover, in Robert's pelvis the flattening is observed when both ossa innominata are shortened, and when, therefore, there is no relative shortening, and therefore no shortening that can be made to account for the deformity.

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* This important distinction of characteristic deformity of the side of the pelvis from the degree of diminution of pelvic diameters, or change of shape of the pelvic passage, appears to me to have escaped the observation of M. Hubert, who has consequently arrived at faulty conclusions.

I must refer the student to the papers of M. Hubert and of Mr. Wood, for the details of their opinions regarding the influence of atrophy or deficient ossific development in producing Nægele's deformity. I have preferred stating my own views in my own way, and so as to bear on the reasonings of these gentlemen, to preparing a methodical and detailed answer to their arguments, which might degenerate into at least the appearance of controversy. I shall only say that the views already expressed regarding the normal mechanism of the iliac beam, in which mechanism the sacro-iliac joint is an essential part—views with which M. Hubert and Mr. Wood nearly altogether coincide —are such as to render it impossible to hold that it can be destroyed without affecting the shape of the pelvis. If the views of M. Hubert and Mr. Wood as to the importance of the atrophied condition are correct, then the anchylosis must be regarded as of little importance; a conclusion, it appears to me, altogether inconsistent with the powerful agency of the sacro-iliac joint in the development of the healthy pelvis. In his diagrams of Nægele's pelvis, Mr. Wood has not represented much atrophy of the sacrum or ilium; and it appears to me that, in any case of oblique pelvis, if the os innominatum retain its unnatural shape, the complete absence of atrophy will not destroy the characteristic obliquity.

Accordingly, as modern research advances, the importance of the joints of the pelvis becomes more and more evident. They are now regarded as contributing not merely to the safety of the structures of the body, by their action, along with numerous other joints, in preventing mechanical injury from various forms of violence; to the prevention of shock to the nervous system from similar causes; to the expansion of the pelvic passage in parturition; but also to the development of the healthy pelvis into its perfect form. In his remarks on the probable cause of oblique deformity, Nægele makes it appear that he suspected that the absence of the sacro-iliac joint was of more importance than he could by any means show it to be. He cites a memoir by Phœbus on deformities connected with congenital anchylosis, and the observation by Tourtual of a young Bohemian of fourteen years of age, whose cranium was deformed, atrophied, and wanting the usual sutures. Tourtual's observation has had the fate of being cited as affording important suggestions by authors, who nevertheless refuse to recognise the importance of the anchylosis in producing Nægele's pelvis and Robert's. Fortunately, it led M. Dubois to push his investigations in a different direction. He had the advantage of examining, in the Museum of Pathological Anatomy of the Faculty of Medicine of Paris, a specimen of anchylosis of one of the squamous sutures, with marked flattening of the cranial vault in the part corresponding to the union. With the assistance of Professor Gavarret, he produced a theory of the action of the sacro-iliac joints in producing the healthy evolution of the pelvis, and also of their absence producing a morbid evolution of the same part. My attention was first drawn to this interesting subject on examining the views of these gentlemen. I soon became satisfied [illegible] importance of the sacro-iliac joints and of their [illegible] equally convinced that the explanation [illegible] Dubois and Gavarret was erroneous, and [illegible] advanced one of my own.* Since [illegible], in his valuable work [illegible] has broached another theory of Nægele's pelvis, founded on certain conditions of the [illegible] joints which he believes to be present in cases where this deformity is being produced. He supposes that one of the joints—namely, that on the so-called healthy side—is more relaxed than the other. The sacrum, unequally supported between its points of suspension, is consequently drawn into an asymmetrical position in relation to the ossa innominata. It is separated from that which helps to form the relaxed joint, and brought nearer to the other. But as the sacrum is, he says, already in immediate contact with the latter, there will be powerful compression of the surfaces touching one another, and consequently more or less serious disturbance of the circulation and nutrition of the compressed parts. As a consequence of this, arrest of development of that half of the sacrum and neighbouring portion of ilium may be expected. On the lower side of the pelvis, or that having the less rigidity of parts in youth, abnormal changes will also take place. In the natural state, each half of the pelvis is subjected to a double force, one tending to increase the curve of the superior half of the circle, the other tending to destroy the natural curvature of the lower half-circle. If the circumstances of all the parts were the same, says M. Giraud-Teulon, the two forces could be considered simultaneously; but if we remark that the degree of their rigidity is far from being the same at different periods of life,—that in the first fifteen years the anterior or lower arch is scarcely solid, while the posterior is already completely ossified, —we will naturally expect to find the second of these forces more frequently producing its result than the other, the cotyloid cavities yielding to the force pushing them towards the antero-posterior vertical plane. In Næegele's pelvis, the symphysis pubis is a point fixed, by its being partly formed by the healthy os innominatum. It is only, therefore, the cotyloid cavity of the affected side that is pushed inwards.

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* See page 78.

The theory of M. Giraud-Teulon, while it is very ingenious and worthy of consideration, seems to me not to demand further discussion at present, on account of its being inapplicable to the conditions at present believed to exist in the pelvis of Næegele which it is intended specially to explain. Instead of different degrees of relaxation of the two sacro-iliac joints, there is believed to be, in cases of Næegele's pelvis, complete absence from the earliest periods of one joint, while the other joint is believed to be perfectly healthy, and has never, so far as I am aware, been shown to be otherwise. This difference between the assumptions of the theory and the phenomena to be explained must be removed before the explanation can be accepted, and renders it unnecessary to point out minor difficulties at present.

It has been very illogically argued, as conclusive against all theories of Næegele's pelvis, in which anchylosis is an essential condition, that cases of oblique deformity occur where there is no anchylosis; and, *vice versa*, that cases of normal pelves occur where there is anchylosis.

It is well known that cases of oblique deformity of the pelvis, more or less resembling Næegele's, are observed without any anchylosis. Such cases often do not very closely resemble the characteristic deformity, and can scarcely do so, seeing that they sometimes do not at the same time present the atrophy so important in it. But, supposing the resemblance in form were exact, and the anchylosis absent, these circumstances would afford no ground for denying the influence of the anchylosis in Nægele's pelvis, unless it were inconceivable that any other combination of circumstances could produce the oblique deformity except those producing Nægele's deformity. This is very far from being the case. But while it is so, I venture to express a belief, that the establishment of the true theory of Nægele's pelvis will soon lead to the discovery of the causation of oblique deformities of all kinds.

It has been truly alleged that cases occur of pelves with sacro-iliac anchylosis, and no further deformity. These cases are not common, and are illustrations of anchylosis occurring after complete development, not before it. If a case can be adduced of anchylosis in early infancy, in which the pelvis has grown, under the usual conditions, into its healthy shape, it might be fatal to the theories in which anchylosis is a postulate. But this is not to be feared.

If the theory of the natural development of the pelvis adduced in a former part of this paper be correct, then it is very easy to account for the occurrence of Nægele's deformity,—a circumstance which, by a reflex action, strongly corroborates the original views.

Congenital absence of the sacro-iliac joint on one side is generally admitted to be an essential character of the deformity. If this, then, be assumed, the explanation of the deformity of shape is very easy. For the iliac beam united to the sacrum, not moving or potentially moving on it, loses all the peculiar mechanical relations which it has in a natural condition. The weight of the body on the affected side is transmitted to the head of the femur from the sacrum through an unbroken continuous bone. It is not conveyed, as in the natural state, to the upper end of the iliac beam; and therefore its action on that part, in tending to draw it inwards, is lost, as well as the contemporaneous opposite action on the lower end. Indeed, it is the loss of this latter that is essential. For the upward and inward pressure of the femur is unresisted, and this great power prevents the expansion of the pelvis which the natural mechanism of the iliac beam produces; and in some cases it may gradually force the bone to bend slightly inwards between the position of the absent sacro-iliac joint and the acetabulum. Not only is the force exerted on the acetabulum unresisted, it is also greatly increased in amount above its natural state. This increase depends on the acetabulum of the affected side being more nearly in the same vertical line with the weight resting on the sacrum, than the same part on the other side. This circumstance may safely be said to make the affected side bear, if the body be evenly balanced on the pelvis, two or three times the amount borne on the opposite side.

This result of the iliac beam being deprived of its natural arrangement produces, as secondary phenomena, the other changes in the pelvis, except perhaps the atrophic conditions, which are probably in most cases congenital defects, but which are not invariably present to a marked extent, and without which the characteristic form is produced. The advancement upwards and inwards of the acetabulum pushes the symphysis pubis towards the opposite side, and aids in throwing the lower end of the iliac beam of that side farther outwards than is natural. These changes assist in producing the appearance of twisting of the pelvis; the anterior surface of the sacrum not looking forwards to the displaced symphysis, but towards the hip-joint of the affected side.

It is to be observed, that as the deformity increases, or, in other words, as one side of the pelvis is expanded and the other is not, the weight on the abnormal side, or force assisting in the evolution of the form of the pelvis on that side, is increased beyond its natural proportion, while on the other side it is diminished. This arises from the sacrum, or weight to be supported, being on the affected side much nearer a vertical line passing through the acetabulum than on the other; a circumstance which must add to the force producing the deformity on the affected side, and indirectly produce, by the pressure through the symphysis, the deformity on the so-called healthy side.

After the discovery and exposition of secondary or efficient causes, it would not displease even Bacon to find reference made to final causes. This course I intend to pursue, believing that, in the present case, the study of final causes is not only important in regard to the arguments generally derived from them, and into which I do not propose to enter, but also useful in the way of confirming the general accuracy of the theory of the efficient causes already detailed.

Nature has provided the human pelvis with two sacro-iliac joints. The most important known functions of these joints are, to secure the normal development of the pelvis, and to prevent the risk of injury to its integrity from external shocks.

When the sacro-iliac joints are absent, their function of assisting in the production of the natural shape of the pelvis is of course annihilated, and no compensating power is introduced by nature to obviate the evil effect of this loss. A deformity, in short, is the result. But it appears to be otherwise with the function of preserving the pelvis from the dangerous effects of external violence. Clinical experience does not afford any information as to the special liability of oblique pelves to fracture; and we can entertain little hope of this point being soon sufficiently illustrated by examples to enable us to extract lessons from experience. But a study of the peculiar form assumed by the oblique pelvis easily discovers in it arrangements which diminish, or perhaps entirely obviate, the evil that might otherwise result from the loss of this second function of the sacro-iliac joints.

The loss of the development function of the sacro-iliac joint issues in the weight of the body being disproportionately largely laid upon the affected side, the sacrum being, in a direct transverse line, nearer a perpendicular passing through the acetabulum of that side than of the other. The weight of the body is liable at any time to be changed from a pressure into a blow, against which this side has not the protection of the joint. But, to resist the blow, it is strengthened by the very deformity. The arch of the side of the pelvis is destroyed, and the blow is conducted to the head of the femur by an almost straight column of bone; or, the blow is inflicted on the end of a lever equal in length to the small distance between the line of force passing through the sacrum and the support in the acetabulum. On the unaffected side, with its large arc-like curve, the increased or unnatural distance between the line of force passing through the sacrum and the acetabulum is compensated for by this very distance leading to an unnatural amount of the force being expended on the other side, as already said.

The transversely-contracted pelvis of Robert has, for its characteristic conditions, anchylosis of the sacro-iliac joints, flattening of the sides of the pelvis, and atrophy of the sacrum and of both ilia. Its morbid conditions may be regarded as being primarily the same as in the oblique pelvis of Nægele; only, both sides are affected instead of one. This renders the pelvis symmetrical. For the natural function of the iliac beam being lost on both sides, the unresisted, and therefore unnaturally powerful, pressure on the acetabulum will affect each side equally, and there will be no displacement of the symphysis towards one side or of the sacrum towards the opposite. This pelvis affords as good evidence as Nægele's of the true mechanism of the iliac beams; for, their action being lost, the pelvis is not at all expanded during its growth, but contracted, and in the adult bears a strong resemblance in shape, as Scanzoni* and others have pointed out, to the pelvis of the fœtus or young child.

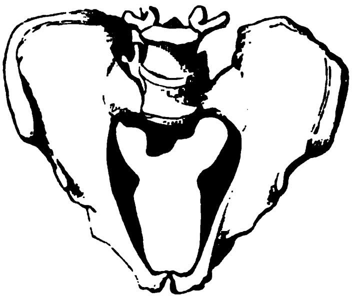

Fig. 16.

I shall not enter upon the consideration of the bearings of this curious form of pelvis on the theory

Fig. 16. Pelvis of Robert, or the transversely-contracted pelvis.

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* Nicht umhin können wir ferner, hinzuweisen auf die grosse Aehnlichkeit dieses Beckens mit jenem des Fötus und des neugebornen Kindes. Betrachtet man eines dieser letzteren, so glaubt man wirklich, mit Ausnahme der erwähnten Synostose, ein querverengtes Becken im Kleinen vor sich zu sehen.—*Lehrbuch der Geburtshülfe*, 1853, S. 561.

of pelvic development which I have advanced. This I have already done incidentally, and the reflection of the intelligent reader will supply what is wanting better than I can. Indeed, to attempt it would require something so nearly like dry repetition, that its avoidance will not be more gratifying to the reader than agreeable to myself. I shall only say, that the whole phenomena of Nægele's and Robert's pelves are explained by and support the theory of the natural development of the pelvis which I have supported, while they appear to me to be irreconcilable with, and therefore adverse to, all the other explanations of the phenomena hitherto advanced.

P.S.—It is my duty to record my obligation to Professor Elliot of Liverpool for some valued criticisms on this subject.

CHAPTER IV.

ON THE PROXIMATE CAUSE OF THE OBLIQUE-OVATE PELVIS.

THE following chapter is translated freely from the "Text-Book of Midwifery" by Grenser of Dresden.* My object in introducing it here is not merely or chiefly to lay before my reader a good sketch of the subject, but to show the difference between what I have aimed at in the previous chapters, and what Grenser succinctly describes as his pursuit, and that of the authors whom he quotes. Grenser seeks the proximate, the pathological or anatomical cause, of the deformity. I seek to show how such cause, whatever it may be, produces the deformity. It will be observed, that, with remarkable unanimity, the cause is described as congenital, or as having left no active osseous or articular disease progressing during the chief part of the growth of the bones. For my discussion, this admission is all that is desired. I wish to show, and hope I have shown, how such pathological and long-past phenomena should lead to the growth in subsequent years of the pelvis into its peculiar shape.

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* *Lehrbuch der Geburtshülfe. Sechste Auflage*, S. 515 : Mainz, 1867.

"Fr. C. Nægele, in his classical monograph of the obliquely-contracted pelvis, had adopted congenital malformation as the most probable cause of this deformity of the pelvis in the majority of cases, and adduced the following grounds for this mode of production:—1. The intimate complete union of the sacrum with the iliac bone, and absence of such marks in the region of the synostosis as might indicate a coalition of parts originally separate; 2. The imperfect development of one lateral half of the sacrum, the smaller breadth of the innominate bone of the same side, and the smaller height of the synostosis in comparison with the synchondrosis of the opposite side; 3. The experience that synostoses and deformities equally in other bones occur as original errors of development, and that congenital synostosis is usually connected with a deformity of the coalesced bones, which for the most part consists of imperfect development; 4. The great resemblance of these pelves to one another; 5. The absence of every other cause or external influence which could give rise to the deformity (Nægele, a. a. O. p. 64). Tiedemann, Rokitansky, Arnold, and others, took the same view of the production of obliquely-contracted pelves. Stein and Martin met them with the assertion that the synostosis could only be a consequence of inflammation, and the latter particularly says—'The union of the iliac and sacral bones, with condensation of the adjoining osseous texture, is the first acquired deviation, when not during fœtal life, then in the first years of childhood, and the ankylosis hinders the further development of the neighbouring parts, and hence will arise the displacement of the bones during their further growth.'—'De pelve oblique-ovata, c. ancyl. sacro-il.' Jen. 1841: Ders. in der *N. Zeitschr. f. Geburtsk.* xv. p. 49, u. xix. p. 111. Martin regards, as specially favouring his view, the pelvis briefly described by Danyau, which belonged to a person who died after confinement, and who had suffered for ten years from coxalgia (*Journ. de Chir.* Mars, 1845, p. 75). Besides the above-cited writings by Martin, Hohl, Hayn, and Litzmann, there may be consulted on the point—Unna, in the *Hamb. Zeitschr. f. d. ges. Med.* Bd. xxiii. H. 3, Moleschott, *Ibid.* Bd. xxxi. p. 441 (who found the bone-substance taken from a sawn-through synostosis of an obliquely-contracted pelvis to be quite normal under microscopical investigation). Kirchhoffer, in the next-cited essay, V. Ritgen, *N. Zeitschr. f. Geburtsk.* Bd. xxviii. and xxx. Hohl settled the literary conflict regarding the mode of production of oblique-oval pelvis on the basis of very numerous and fundamental researches, on the skeletons both of fœtuses and children, and adopting three forms of oblique-oval pelvis: 1. Congenital oblique-oval pelvis, produced by a completely-restricted formation or deficient development of the osseous centres, for the alæ of the sacrum on one side, with or without the subsequent addition of synostosis of the sacro-iliac joint, but this in no case the result of inflammation; 2. Oblique-oval pelvis, produced in earliest childhood by deficient production and development of the alæ in consequence of similar causes, as in fœtal life, or in consequence of intimate diseases—for example, rachitis, scrofula, atrophy—with or without the subsequent addition of synostosis of the sacro-iliac joint, but this not produced by inflammation; and 3. Oblique-oval pelvis, congenital, or produced in the earliest childhood, with the subsequent addition of synostosis of the sacro-iliac joint produced by inflammation, etc. In this way had Hohl partly corroborated, partly extended by new facts, the view of Nægele. It is particularly important to note that he has distinctly shown that absence or deficient development of the ala of the first sacral vertebra on one side really occurs as an original error of development; that this exerts no special influence on the ilium and the construction of the pelvis, if the ala of the second sacral vertebra has undergone an unusual extension, and receives the entire auricular surface of the ilium; that, however, the original participation of this, and perhaps also of the third ala, in the faulty formation of the first, exerts so much the more influence upon the development of the ilium, the greater it is; and that then the oblique-oval shape is gradually produced without any diseased action being recognisable in the pelvis. The question was in this state up till 1861, when the comprehensive monograph of Thomas on the obliquely-contracted pelvis, which is worthy of being placed beside the classical work of Nægele, came with convincing arguments to the support of the view first propounded by Ed. Martin regarding the production of this deformity of the [illegible] 156. [illegible] al. [illegible] which [illegible] above- [illegible] classified [illegible]

[illegible] regarding which [illegible] were [illegible] analysis [illegible]

[illegible] traces of [illegible] cases [illegible]

[illegible] traces of [illegible]

[illegible] results [illegible] same of [illegible]

[illegible] traces of [illegible] and [illegible] carried [illegible] of the [illegible]

[illegible] visible [illegible] and regarding which the [illegible] is sufficiently [illegible] statement that they had not [illegible] from [illegible] of the bones of the [illegible]

From this collection it appears that 19 cases are in opposition to Nægele's view, 27 give evidence neither *pro* nor *con*, and only 4 can at last be brought to its support. 'But,' says Thomas, 'does not this support lose all its force, if one reflects that diseases of the bones frequently end imperceptibly and without marks, that many old people of the lower orders can recall to mind with security almost nothing of the diseases of their children, and that the narratives which can be procured from the persons themselves are thoroughly unreliable in this point of view, because in every case they no longer have recollection of what befell them in childhood?' In conclusion, Thomas propounds the following corollaries:—

1. In every obliquely-contracted pelvis, ankylosis is to be regarded as the primitive fault and as an acquired evil.

2. For the production of the ankylosis, inflammation of the ilio-sacral joint is requisite.

3. Thus, inflammation may occur at any age and even during fœtal life; in this way an obliquely-contracted pelvis may be congenital.

4. The inflammation may arise from internal causes originating in the ilio-sacral joint, or it may be the consequence of a traumatic lesion, or it may occur, secondarily, after a disease of a neighbouring joint—for example, of the joints of the lumbar vertebræ or hip.

5. The ankylosis has, as a consequence, an atrophy or a shrinking of the abnormally-united bones.

6. The earlier the ankylosis takes place, the more completely is the deformity produced.

7. The absence of the ala of the sacrum, in the cases in which the ankylosis was early produced, is only apparent.

8. If the ankylosis occurs after puberty has already made its appearance, and the bones of the pelvis have reached their complete growth, then there is only to be seen a shrinking of the adjacent parts of the bones.

9. This osseous atrophy or shrinking is analogous to that which is observed in ankylosis of the movable joints.

10. After cure of the primitive disease which leads to the ankylosis, the marks of the former joint may be so completely obliterated that they cannot be made out on superficial examination, and can sometimes be clearly made out only after cutting through the united bones.

11. The remaining deformities, apart from the ankylosis—for example, the obliquity and contraction of the pelvic canal, the flattening of the lateral wall of the pelvis, the smallness of the great sciatic notch, the curvature of the lumbar spine, etc.—are secondary, and must be partly explained by the osseous atrophy, partly by the unequal pressure to which the two lateral halves of the pelvis are subjected, partly by the necessity of restoring the lost equilibrium.

When in oblique-oval pelves coxalgia of one side ccurs, Litzmann regards this as a common cause of e obliquity, as well as the ankylosis, and adduces for this view very weighty reasons, to which we shall in another place recur. In a later work (*Monatsschr. f. Geb.* Bd. xxiii. p. 249) Litzmann—in opposition to Simon Thomas, who holds that in all cases the ankylosis is primary and essential and everything else secondary—tries to prove that the ankylosis of the sacrum with the ilium is secondary, a partial result of an overpowering pressure from the acetabulum. According to Litzmann, the cause of the obliquity is the same in all obliquely-deformed pelves—namely a persisting and overpowering pressure directed against one-half of the pelvis, the weight of the trunk chiefly or exclusively falling upon the extremity of this side. The conditions only, under which this one-sided pressure comes to pass, are different, as—1. Lateral curvature of the spine, mostly of rachitic origin; 2. Difficult or completely-suspended use of one lower extremity, most frequently in consequence of coxalgia of one side; 3. High degree of want of symmetry of the sacrum, produced by incomplete growth and development, or by atrophy of the ala of the sacrum on one side. The presence or absence of synostosis of the sacrum with the ilium, on the side where the pressure is, causes no essential difference. Olshausen also opposes the view of Simon Thomas as too exclusive, communicates a new case of obliquely-contracted pelvis with sacro-iliac ankylosis, and tries to show that sacral deficiency is the primary ætiological cause of this kind of pelvis, the synostosis the consequence of the one-sided strong pressure from one hip-joint (*Mo-*

natsschr. f. Geburtsk. Bd. xix. p. 161). Simon Thomas still defends his opinion against these remarks (*Ibid.* Bd. xx. p. 384). Lastly, Ed. Martin relates a successful case of delivery in an obliquely-contracted pelvis, with ankylosis of the left synchondrosis and overgrowth of bone on the body of the uppermost sacral vertebra, and finds in this latest observation the fullest confirmation of his view that inflammation causes the ankylosis (*Monatsschr. f. Geburtsk.* Bd. xix. p. 251)."

CHAPTER V.

ON THE PELVIC ARTICULATIONS IN PARTURITION, ETC. ETC.

In the lower animals we find beautiful examples of the changes taking place in the pelvic joints in the end of pregnancy. And the intimate nature of these changes is most satisfactorily studied in these animals, because in them it is comparatively easy to procure specimens of the altered tissues at any period, and still more so, because in many of them the changes are to an extent far exceeding what is ever found in the human female. In illustration, I may at present cite the changed condition of the pelvis of the guinea-pig and of the cow. In the former there takes place, at the time of parturition, a very considerable separation of the pubic bones; the ligamentous tissue stretching, in this small quadruped, to the extent of an inch, or even more. This enlargement of the pelvic circle, by separation of the pubic bones, necessarily implies great relaxation of the tissues on the anterior or inferior part of the sacro-iliac joint, and freedom of motion in it. After parturition the pubic bones again become closely united.

In the cow, the changes in the pelvic joints differ in some important respects from those just described as occurring in the guinea-pig. In the latter, it has been stated that the most notable change is the elongation of the ligaments of the symphysis pubis (nature in this way foreshadowing the operation of symphyseotomy), and the separation of these bones giving rise to corresponding motions in the iliac bones, analogous to the abduction of the limbs. In the cow, on the other hand, these movements are completely absent. The symphysis pubis is consolidated by bony union, and thus incapable of distraction, and consequently the abduction of the iliac bones is impossible. But, nevertheless, the changes in the cow's pelvis are of great importance. They have been described by Professor Barlow of the Veterinary College,* and I have demonstrated them upon Mr. Barlow's preparation to the members of the Edinburgh Obstetrical Society. They consist in an increased development of the large sacro-sciatic ligaments, which, from being of moderate thickness and in a state of tension in the non-pregnant cow, become much increased not only in thickness but also in length, and are thus made slack and yielding. The tension of these ligaments tends to fix the sacrum and consolidate it with the ilia, and their relaxation leaves it freer to move. Further to facilitate this motion, the sacro-iliac joints, which in the non-pregnant cow are described by Mr. Barlow as secured by a material closely resembling intervertebral substance, now have the opposing bony surfaces smooth and lubricated, and the surrounding fibrous ligaments relaxed. By these changes the ilia become extensively movable upon the sacrum (or *vice versâ*) in an antero-posterior direction, the motions being analogous to those of flexion and extension in the limbs. The final result of these changes and motions is to enlarge the genital passages in this animal.

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* *Monthly Journal of Medical Science*, January 1854, p. 83.

It has hitherto been customary to regard the articulations of the pelvis in man as virtually immovable, and to describe cases where motion evidently takes place in the female, at the time of parturition, as morbid in their character.[*] But Mr. Zaglas[†] has pointed out that in man there is distinct motion of the ossa innominata in an antero-posterior direction, or upon an imaginary line passing transversely through the second sacral vertebra from one side to the other. In other words, the sacrum may be described as having a nutatory motion upon this imaginary transverse axis, the promontory of the sacrum advancing downwards and forwards, while its apex moves in a contrary direction, and *vice versâ*. In the downward motion of the promontory, which in the non-pregnant is to the extent of about a line, the brim of the pelvis is diminished to the same extent in its conjugate diameter, while the corresponding upward motion of the apex of the bone, to the extent of about two lines, puts the sacro-sciatic ligaments on the stretch, and enlarges the dimensions of the outlet. By observations on the living and on the dead subject, Mr. Zaglas has shown that in the erect position the sacral promontory is not in the position of greatest projection into the brim of the pelvis, but the reverse, and consequently that the apex is in its forward position diminishing the outlet, and relaxing the sacro-sciatic ligaments. When the body is bent forward, on the other hand, the base of the sacrum is protruded into the brim, the apex is tilted upwards, the sacro-sciatic ligaments put on the stretch, and the outlet of the pelvis consequently enlarged. These movements take place ordinarily in both man and woman, in defæcation, etc., but in her they are of greatest interest and importance in the function of parturition. Before entering on this part of the subject, I shall first point out some peculiarities in the pelvic articulations in woman, and describe the changes taking place in them in the end of pregnancy.

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[*] For authority see Wood, art. "Pelvis," Todd's *Cyclopædia of Anat. and Phys. Suppl.* p. 14. See also Baudelocque, *System of Midwifery*, Heath's Transl. vol. i. p. 33, who refers to Louis, "Dissertation sur l'Ecartement des Os du Bassin," *Memoires de l'Acad. Roy. de Chir.* tome iv. See also Lachapelle, *Pratique des Accouchements*, tome iv. p. 85. For an example of an old author denying mobility even in labour, see the works of Realdus Columbus, quoted in Siebold's *Geschichte der Geb.* ii. Band, S. 47.

[†] *Monthly Journal of Medical Science* for September 1851, p. 289.

The three large pelvic articulations present the following important peculiarities in regard to their mode of union:—Each articulating surface of bone presents two distinct parts; the one comparatively smooth, covered with cartilage, and only partially united to its neighbour; the other rough, for the attachment of very strong and numerous bands of fibrous and fibro-cartilaginous tissue, firmly uniting it to the corresponding surface of its neighbour. The former surfaces form the anterior and inferior parts of the sacro-iliac joints, and in the skeleton are known as the auricular surfaces. In the symphysis pubis these surfaces form the superior and posterior parts of the joint. Interposed between the investing cartilages at these parts is a synovial bursa. It is on these surfaces that the articular motion is most free, the ulterior advantages of which, in the physiology of the erect position, etc., this is not the place to demonstrate.*

Mr. Zaglas points out that on the os innominatum we may divide the entire articular surfaces into four parts. The two antero-inferior of these correspond to what is called the auricular surface, and are inclined to one another at an angle which looks outwards, and forms a ridge inwards. The two posterior superior surfaces (which are separated by a large mass of fibrous ligamentous tissue from the corresponding surface of the sacrum) are inclined to one another at a similar angle; and there is accordingly a crooked ridge running between the four surfaces in a direction from the spine to the symphysis. Taking the four surfaces, however, in another relation, it may be seen that the two posterior superior surfaces are separated from the two anterior inferior by a groove (running nearly parallel with the axis of the sacrum), and are inclined to them at an angle which looks inwards. The consequence of this arrangement is, that while some motion is permitted, any tendency to displacement is entirely obviated, so long as the innominate bones are retained in a due degree of proximity by the pressure on the acetabula, and by their ligaments.[*] But, besides these just remarks on the general arrangement of the whole joint, it is important to notice another striking peculiarity always observed, and frequently in a very marked degree. This consists in the existence upon the posterior articular surface of the ilium of a bony prominence of irregular outlines, but frequently assuming the form of a solid projecting angle. This projection is found to correspond to a distinct cavity on the opposed surface of the sacrum, which is, in some of its functions, analogous to a cotyloid cavity. For upon these parts (which are generally on a level with the upper part of the second bone of the sacrum) the motions of the ilia must take place; and whilst they will offer no absolute resistance to the motions of the ilia upon the sacrum, analogous to flexion and extension in the limbs, they will, like the general arrangement of the entire articular surfaces of the articulation, tend to prevent the slipping upwards or downwards of the one bone upon the other, motions which would necessarily prejudice the security of the erect position.

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* In a specimen I exhibited to the Obstetrical Society there is a double synovial bag in the symphysis pubis. This joint was removed from a virgin about eighteen years of age. For a fuller exposition of these anatomical points see the *Traité des Accouchements* of P. A. Dubois; also Mr. Zaglas's "Observations on the Symphysis Pubis," in *Monthly Journal* for November 1851, p. 489.

In the latter half of pregnancy the soft tissues contributing to form the pelvic joints are invariably, or almost invariably, found softened as if by serous infiltration; and the joints are consequently relaxed. Most anatomists and obstetricians,* who have paid attention to this subject, agree in this statement. The softening of these tissues is generally accompanied by their increase in thickness, a change which will in itself have, as a necessary consequence, the separation of the bones, and the enlargement of the pelvic circle.† And I have no doubt that this favourable circumstance, together with others connected with the motions of the joints to be presently discussed, forms an important part of the explanation of some cases of delivery, by a simpler operative procedure than may have been predicted to be necessary. Indeed the experiments of MM. Giraud et Ansiaux‡ seemed to them to show that in contracted pelves this change in the joints takes place to a greater extent than in well-formed pelves. In some cases the thickening of the tissues goes on to quite an extraordinary extent. Boyer states that in one case he found the sacro-iliac joint separated to the extent of half an inch; Chaussier found the symphysis pubis separated still more in an easy labour, and Madame Boivin asserts that she sometimes found the pubic bones separated to the extent even of an inch. In some cases, as in those of Smellie, Diemerbroek, and Denman, the separation appears to have taken place chiefly during the course of a difficult labour.

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[*] See Report of the Physiological Society, *loc. cit.*

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* See Burns' *Principles of Midwifery*, p. 8: Velpeau, *Traité des Accouchements*, Bruxelles, p. 122; also Moreau, *Traité des Accouchements*, tom. i. p. 40.

† See Luschka. Virchow's *Archiv.* Bd. vii. 1854.

‡ Jacquemier, *Manuel des Accouchements.* Tom. ii. p. 476.

But although there can be no doubt as to the thickening and softening of the tissues forming the pelvic joints, there is great difference as to their capability of motion. In this country, indeed, most authors seem to think that motion in these joints, in pregnancy, is always to be considered the result of a morbid process. This opinion is, without doubt, erroneous, although there are observed, not unfrequently, cases where the natural relaxation of these joints increases to such an extent as to interfere with the function of progression.*

The observations which I have already made upon the movements in the pelvic joints in the non-pregnant, set aside at once all the arguments adduced to show that there is naturally no such mobility in pregnancy, and that when movements are observed in these joints a morbid condition exists. Founding upon what has just been stated as to the condition of the ligaments of the pelvic articulations in the latter part of pregnancy, we can, without difficulty, assert that at that time the pelvic bones enjoy freer and more extensive movements than at other times. In very numerous cases, scattered through obstetric literature, where these joints have been examined after delivery, authors have described the mobility of these articulations, sometimes, indeed, as being to a very great extent. In addition, cases are not very unfrequent where these movements, either from their freedom and extent, or from their causing pain, attract the attention of the patient and physician. They are then sometimes easily perceived on making the proper examinations.

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* Some extraordinary observations have been made by obstetricians in regard to certain motions of these bones in difficult labours, and under the influence of the pressure of the presenting part. For instance, Madame Lachapelle mentions a case where one ilium became dislocated forwards upon the sacrum, so as to enlarge the oblique diameter of the pelvis, through which the head was passing. Others have described a similar dislocation of both bones simultaneously. A case is recorded by Breit. See Luschka, *loc. cit.* See also Smellie, *Collection of Cases and Observations in Midwifery*, vol. ii. p. 4; and Hyrtl. *Topograph. Anat.* Bd. ii. S. 14.

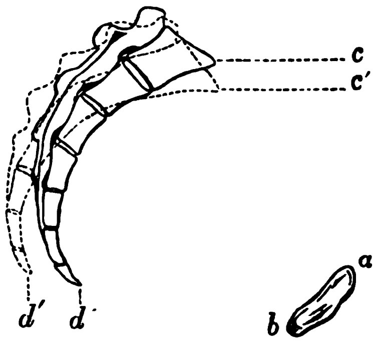

Fig. 17.

The movements which occur may be described as consisting in the elevation and depression of the symphysis pubis, the ilia moving upon the sacrum; or if the sacrum be regarded as the moving bone, it

Fig. 17. Showing the nutatory motion of the sacrum in parturition. *a b* is the symphysis pubis which is supposed to be fixed. *c d* is the sacrum in its ordinary position. *c′ d′* is the sacrum at its extreme of nutation, the promontory being advanced forwards, and the apex correspondingly projected backwards, so as to enlarge the outlet.

describes a nutatory motion upon an imaginary transverse line passing through the second bone. By the elevation of the symphysis pubis (or nodding forwards of the promontory), the angle of inclination of the pelvis is lessened, and the conjugate diameter of the brim of the pelvis is diminished to the extent of one or even two lines (Fig. 17); the corresponding diameter also of the outlet is increased, probably about twice as much. This different ratio of the effects of the motion upon the brim and outlet results from the fact of the centre of motion being much nearer the promontory than the apex of the bone. The promontory, therefore, will describe an arc of a smaller circle than the apex.*

That the alteration of the dimensions of the brim and outlet by these movements is not insignificant, but the reverse, is a proposition which every obstetrician will confirm. It only remains, then, to be observed how these alterations correspond with the phenomena of the progress of the child in parturition. Now it has been already stated, that in the erect position the brim of the pelvis is in its enlarged condition, the symphysis pubis being then depressed, while the outlet is correspondingly contracted. Now in the course of the first stage of labour, while the head is pressing into the brim, the human female is generally standing, sitting, or lying on her back, or in an easy position. But as soon as the head has descended into the pelvis and impinged upon the sensitive vagina, then forcing efforts accompany the pains. These forcing efforts consist, in great part, of powerful contractions of the anterior abdominal muscles, the effect of which, especially the action of the two recti muscles, will be to tilt up the symphysis pubis, thus throwing the promontory forwards, contracting the brim, and enlarging the outlet, and diminishing the angle of inclination of the pelvis. To all these changes the position usually assumed by the female in the second stage of labour will contribute. For it has already been stated, that the simple bending of the body forwards has for its effect the tilting upwards of the apex of the sacrum and enlarging of the outlet. And it is a curious fact, that a woman in her forcing pains, in the second stage, is found to draw up her legs, and bend her body forwards, thus inducing changes in her pelvis which facilitate the advance of the child in that stage.*

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* This motion of nutation of the sacrum is not described by Luschka, *Virchow's Archiv*, Bd. vii. 1854, nor by Schwegel, *Monatsschrift fur Geburtsk.* 1859. For a diagrammatic representation of this motion, see Wood, article "Pelvis," Todd's *Cyclopædia of Anat. and Phys.* supplementary volume, p. 145.

The motions of the pelvic bones, which we have been detailing, agree exactly with those which take place in the cow in parturition. In that animal the first effect of each pain is to elevate the tail, and thus enlarge the outlet for the escape of the calf.

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* Another motion is described by Laborie, namely, separation of the ischiatic tuberosities. But it appears to me to demand further inquiry, before being accepted as true. See *Gazette Hebdomadaire de Med.*, No. 34, 1862.

The mechanism we have just been describing in the human female is analogous to that which we have previously shown to occur in the pregnant and parturient cow. The changes which occur at the time of labour in the guinea pig find their analogues in the altered conditions of the symphysis pubis in the human female. But in her they are only to a small degree comparatively. It is important, however, to remember, that in this joint the thickening of the ligaments is generally more apparent than in the sacro-iliac joints. The distension of the pubic ligaments will be easier in the recumbent than in the erect attitude, which last implies strong compression of the tissues of the joint by the pubic bones. Moreover, the separation of the thighs, which is habitually practised at the latter part of a labour, will favour any possible slight separation of the pubic bones, especially if the internal femoral muscles are in a state of contraction while the thighs are apart. Indeed, the study of the whole subject illustrates beautifully how nature leads the human female, in the act of child-birth, to assume positions and make exertions which are necessary for perfecting the mechanism of the process.

There is another source of information as to the state of the pelvic joints, especially the sacro-iliac, namely, the results of section of the symphysis pubis, an operation which appears to me to have been prematurely abandoned. In the performance of this operation upon the living female in order to aid in delivery, it has been found that the pubic bones can be separated to the extent of from one and a half to two or even three inches, without any damage to the sacro-iliac joint, or with the result of merely lacerating the capsular fibres of the anterior part of the articulation. In many of the cases the pubic bones, after the division of the symphysis, seemed to part from one another with resiliency, as if their union counteracted some force tending to separate them. This phenomenon admits of two explanations: either by attributing it to the weight of the limbs acting upon the acetabula, or by ascribing it to the elasticity of the great mass of elastic fibrous tissue in the posterior and upper parts of the sacro-iliac articulation, which is not resisted by the different mode of union in the anterior and lower part of the joint as already described. In the pelvis of the male and non-pregnant female, when this operation is performed after death, it is found that a separation of from one, in some cases, to even two inches can be effected without injury. These facts illustrate the relaxation of the sacro-iliac joints at the end of pregnancy. They are, however, perhaps of more importance in regard to symphyseotomy, as showing the amount of separation that may be produced without injury. But the dread of injury is probably, in the main, a groundless fancy; for we know that to destroy the sacro-iliac joint, after section of the symphysis pubis, it is necessary to use great force, a circumstance which is, in itself, the best guardian of the safety of the joint.

The operation of symphyseotomy, as reintroduced to the profession in 1768 by MM. Sigault and Le Roy, is one which has, with justice, been condemned. But the jealousy of the Academy of Surgery, which discountenanced M. Sigault's operation at the first, led the members, after the subsidence of the excitement produced by its first and only occasional successes, to repeat their condemnations of it, and prevented its obtaining a fair consideration. It yet remains to be seen whether the operation, as more broadly proposed, long before Sigault, by Severin Pineau,* may not be one which is destined to have a place among the operations of practical midwifery, devoted to saving the life of the unborn child. In this country, the operation received, after its proposal by Sigault and Le Roy, the high sanction of W. Hunter and Denman, so far as its own peculiarities were concerned. But they, at the same time, showed that it could be of very little, if any, service in the cases for which it was proposed, namely, those of extreme pelvic distortion where Cæsarean section would otherwise be required. In this condition matters have been allowed to rest. British obstetric authors have loaded the operation itself with calumnies which are quite unfounded, and raised difficulties about it which are sufficient to deter a superficial inquirer from its consideration.

There is every reason to believe that the operation, n itself, is one of slight danger at the time, or even

udelocque's *System of Midwifery*, Heath's Transl. vol. iii. p. 238.

ulteriorly, if compared with the dreadful results of craniotomy and Cæsarean section. For the latter operation it can very seldom be a substitute. But it remains to be seen whether the former—namely, craniotomy—may not in some cases be superseded by it. There is every reason to think that the operation would be much less dangerous to the mother than craniotomy, even with the allowance of great freedom in the selection of cases; and it would give a chance of saving the child, whose life is necessarily compromised by that proceeding. Moreover, the operation might probably be simplified by adapting to it the subcutaneous method, and other improvements.

I conclude these remarks with the following quotation from the most esteemed author in British obstetrics, whose name and influence have contributed greatly to the neglect into which the operation has fallen:—

"It is proved," he says, "in the first place, that some enlargement of the capacity of the pelvis is actually obtained by dividing the symphysis of the ossa pubis.

"Secondly, that the evils which have followed this operation have been very much occasioned by its being performed unskilfully, or by injudicious endeavours to increase that enlargement of the capacity of the pelvis beyond the degree which naturally follows the division of the symphysis.

"Thirdly, that many women who have undergone this operation have recovered; though of those who recovered, many suffered very serious complaints for a long time, or for the remainder of their lives.

"Fourthly, that some children were born living when this operation was performed.

"We may, therefore, presume to say that if a case could be so precisely marked that there should only be a deficiency of just so much space as would be supplied by the simple division of the symphysis, the operation might in that particular case be considered.*

"We may also say, that this operation is not so certainly fatal to those women on whom it may be performed as the Cæsarean operation; nor so certainly destructive of children as that of lessening the head.

"We may, then, be allowed to suppose a case, and such a one is more than possible, in which a person of very high rank, the life of whose child might be of the greatest public importance, could not be delivered without the destruction of the child, or her child be preserved but by the Cæsarean operation at the expense or great hazard of her life; and that she, through human frailty, might refuse to submit to the Cæsarean operation, yet the great interests and policy of the nation might forbid the destruction of the child. Of course both the mother and child would be inevitably lost. Should such a case occur, which, as I said before, is more than possible, then the section of the symphysis of the ossa pubis might be proposed and performed, as it would in some measure meet both these interests; being less horrid to the woman than the Cæsarean operation, and, instead of adding to the danger, give some chance of preserving the life of the child."*

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* "The confidence of M. Sigault in this new method was so diminished in the latter periods, that he declined performing it when the pelvis had not at least two inches and a half in the small diameter of its entrance."—Baudelocque's *System of Midwifery*, Heath's Translation, volume iii. p. 242. For Baudelocque's and Lachapelle's views of the utility of the operation, see Lachapelle's *Pratique des Accouchements*, tome iii. p. 428, 430, etc.

This testimonial from the eminent and sagacious Denman is the more extraordinary, as he is an author who joins strongly in the cry against the operation, and expressly says, in regard to the above passage quoted from his own work on Midwifery, that he does not "mean to insinuate a wish or advance an argument in favour of this operation, in the cases for which it was originally proposed, or any other which can be imagined."

The last paragraph of the passage just quoted gives in few words a general notion of the cases to which this operation may yet be adapted. But it must be remembered that, in our day, a section of this class of cases has already been provided with a suitable treatment in the operation of premature labour; an operation, however, whose use is not inconsistent with the simultaneous use of symphyseotomy.

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* I am not disposed to adopt Denman's morals as displayed in this passage. One treatment for the rich or great and another for the poor or humble, is a principle perhaps often acted on but seldom avowed. In his *Clinical Notes on Uterine Surgery*, Marion Sims adopts a like view, recommending that experimental operations be made on hospital, not on private patients.

PART III.

ON SOME POINTS IN THE PHYSIOLOGY AND PATHOLOGY OF PREGNANCY AND THE PUERPERAL STATE.

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CHAPTER I.

MENSTRUATION IN EARLY PREGNANCY.

AT least since the time of the Hunters, physiologists have taught that one of the earliest results of successful impregnation is the formation or deposition of a new membrane in the cavity of the uterus. This membrane, the caduca of Harvey, and to which William Hunter gave the name of decidua, consists of two chief parts, entitled respectively decidua vera and decidua reflexa; the former, lining the cavity of the uterus internally, and at the edge of the placenta uniting with the latter, which covers the chorion externally. The reflexa has generally, after Bojanus, been described as produced by the intrusion of the ovum from the Fallopian tube upon the vera, which had previously been deposited over the whole inner surface of the uterus, closing up the tubes. By this intrusion the decidua vera was separated, and the separated portion formed the first appearance of the decidua reflexa.

Such were the generally-received opinions for a long time after the Hunters. But the numerous varieties of statement and hypothesis on the part of authors might have been sufficient to show that they were not well founded, for scarcely a single point can be discovered in the history of these parts upon which authors are agreed. For instance, this membrane has been successively described as being vascular and not vascular; as having no openings, or three openings, or only one corresponding to the cervix; as being simple or composed of two, three, or more layers; as being an empty sac, or containing a fluid called by Breschet hydroperione.

John Hunter described this membrane as resembling a layer of coagulable lymph, and originated several other like fanciful and erroneous notions in regard to it. These have frequently been attributed also to his brother William, and this error has been confirmed by the comments of the nephew of the latter, Matthew Baillie, who undertook the office of his posthumous editor. On examining the writings and other works of William Hunter in regard to the decidua, it becomes evident that he had difficulties in regard to it which he did not neglect to state. But all that he does say is correct in every particular, although far from being so explicit or full as might have been, or as he himself evidently desiderated.

The remarks of Dr. Rigby[*] on this subject I have been able to confirm by the perusal of a volume of MS. notes of W. Hunter's lectures preserved in the library of the Royal College of Physicians; and the examination of plate 34th in his great work,[†] affords the amplest evidence to the same effect.

We find him always accurately describing the decidua as the inner layer of the uterus, as having three openings, corresponding to the tubes and os uteri, which are still pervious after the descent of the ovum (*vide* Fig. 7 tab. xxxiv. of his plates), as having an abundant vascularity, and its inner surface perforated like Brussels lace. In accordance also with the plate referred to, he describes the ovum as being "not in the cavity of the uterus, but only between the membranes."[‡] Unfortunately these corrrect descriptions of W. Hunter were neglected, and supplanted by the fanciful doctrines that have been, till recently, everywhere received. And when we consider the names which have lent authority to these views, as well as the rarity of opportunities of examining the parts in question, *in situ*, in healthy subjects; and further, that most observations of the kind have been made on abortions, or, in other words, on abnormal productions, we shall not be astonished at their long maintaining a position in science.

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[*] See Dr. Rigby's edition of "*An Anatomical Description of the Human Gravid Uterus*," etc., by W. Hunter, M.D. etc., p. 48.

[†] *Plates of the Anatomy of the Human Gravid Uterus.*

[‡] *MS. Notes of Lectures*, in the Library of the Royal College of Physicians. "The cavity of the uterus is external to the decidua reflexa, —between it and the decidua vera."

Seiler, C. Weber, and other continental physiologists, were the first to lead the way to more correct views of the nature of the decidua. They opposed the opinion of John Hunter and his numerous followers, that it resembled coagulable lymph in its formation, and described it as a peculiar development of the internal membrane of the uterus. Subsequently E. Weber, Sharpey, Reid, Goodsir, and others, demonstrated the glandular structure of the decidua, and Dr. Sharpey, in particular, threw much light on the whole subject by his investigations into the anatomy of the corresponding parts in the bitch. It is from 1842, however, when M. Coste communicated to the Academy of Sciences the results of his investigations, that we date the complete remodelling of our views on this subject. In 1846 I had an opportunity of examining M. Coste's preparations, and since then have made several observations in regard to this subject.

The preparation (figured in the woodcut) beautifully illustrates several points in regard to the decidua. After describing it briefly, I shall, founding on this and similar observations, proceed to some deductions in regard to points in obstetrics which they unite to explain.

The woman, from whom this preparation was taken, died from causes quite unconnected with the uterus or its functions. Judging from the development of the fœtus, she had arrived at least at the eighth week of pregnancy. The mucous membrane lining the uterus, or the decidua vera, is seen to be of great thickness,

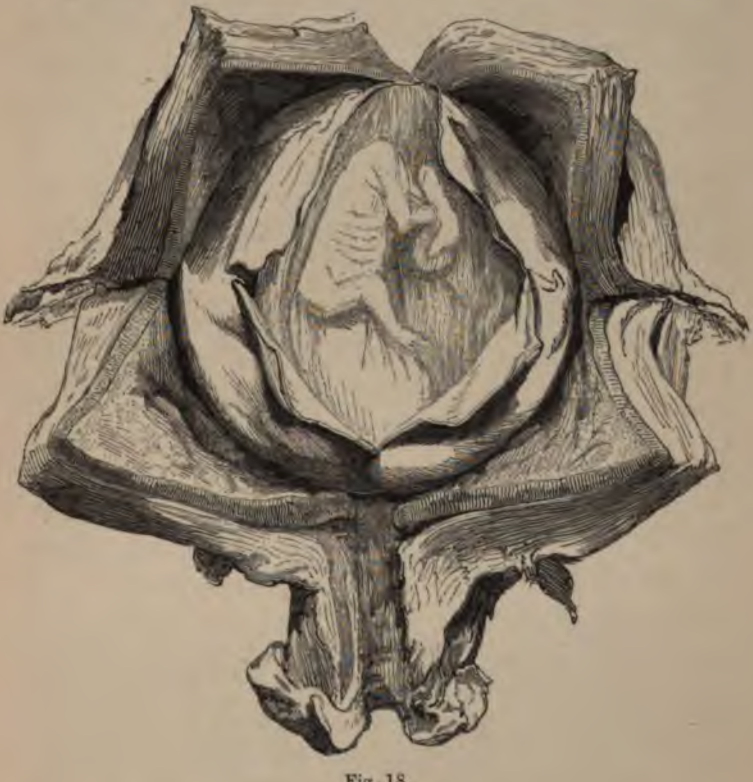

Fig. 18.

Fig. 18. Showing the cavity between the decidua vera and reflexa, also the open state of the cervix uteri and of a Fallopian tube. In order to have the whole drawing included in the page, the size is considerably reduced from that of the original. The decidua reflexa has been opened by a crucial incision and the four flaps turned aside. The fœtus is seen through the membranes; its superior and inferior extremities are well developed. The cavity of the cervix is filled up with the mucous plug.

that lining the cervix is unchanged. The openings of the cervix uteri, and of the Fallopian tube of the left side, are easily seen: that of the right tube has been destroyed in dissection. The decidua reflexa completely covers the ovum, being a thin layer without glands at its most projecting part, and springing from the decidua vera. The cavity of the uterus is still not closed, ample space being left all around the ovum between the two parts of the decidua, from the os uteri to the tubes.

I may here refer to another woodcut (Fig. 19, p. 172), being a reduced representation of one of the most beautiful drawings in Coste's great work.* It shows the open state of the os uteri, and of a Fallopian tube, in a case of uterine pregnancy advanced to the third month. Coste also states, in regard to it, that the cervix contained no plug, such as is ordinarily seen there.

On examining the uterus of a woman dying during or soon after menstruation, we find the mucous membrane of its cavity vascular, much injected, red in colour, soft and permeated by its numerous tubular follicles, having sometimes the appearance of white lines in a cross section. It is, in fact, not capable of being distinguished, except by the minor degree of its evolution, from the decidua of pregnancy. We have thus, at every menstrual period, a nidus prepared for the ovum about to be expelled, and to be made available as a nidus, should the ovum become impregnated.

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* *Histoire générale et particulière du développement des corps organisés.*

The human female, thus, at every menstrual period, passes through the first or initial processes of pregnancy.

Now, whilst most obstetric authors of repute have with great reason doubted or denied the occasional presence of real menstruation during the latter months of pregnancy, they have very generally admitted its occurrence, frequently once, more rarely twice, or thrice, after conception. But at the same time they have felt the difficulty involved in admitting its derivation in any case from its ordinary source, seeing that they believed the uterus to become sealed up by the decidua, or otherwise, immediately after conception.

This sealing-up of the os uteri is of course assumed to take place when the decidua is formed, by those who hold, or rather held, the view that the decidua vera is a closed sac. It is, in truth, sealed up only in those cases in which the ovum has descended to the os to be inserted over it. Another kind of sealing up of the os uteri is frequently described by authors, and is no doubt generally the real condition. This is the closure by the cervical plug of pregnancy; in other words, by the inspissated cervical mucus, which at length becomes dense, sometimes opaque and tough, from its not being, under ordinary circumstances, displaced and renewed in pregnancy, as is the case in the unimpregnated state, at the menstrual periods, and at other times. Weitbrecht's description of this may be here quoted: "Ex compressa cervicis figura, et ex muco isto lento, tenaci, totam cavitatem et omnia ejus foraminula ab uno osculo ad aliud obsidente certe meo quidem judicio, colligitur: uterum prægnantem perfecte clausum esse; omnem igitur introitum vel aëri vel alii cuipiam humori denegari, nullam igitur superfœtationem fieri posse in systemato vermiculari, neque etiam in non prægnantibus semen in uteri cavitatem ascendere posse, quia idem mucus in omnibus aliis cervicum osculis, saltem externis adest."* But there can be no doubt that this plug of mucus is often displaced and discharged in pregnancy. Sometimes, indeed, in *post mortem* examinations, it is found not denser or less movable in the pregnant than in the unimpregnated; and in the dissection by Coste, the figure of which is given on page 172, that author remarks that the cervical mucus or plug was entirely absent, and this in a case of healthy pregnancy. "Were this mucus," says Kussmaul,† "a hindrance to the penetration of semen, the maintenance and multiplication of the human race would be a more difficult business than it has hitherto proved."

The preparations depicted give anatomical proof of the possibility of menstrual fluid being naturally or easily derived from the lining membrane of the cavity of the uterus, up till the end, at least, of the second month of pregnancy. It would only be necessary to have the mucous plug in the cervix displaced, of which so much notice is made by obstetricians. Now it is quite possible, nay, probable, that the persistence of this plug may sometimes be even injurious, by retaining the sanious or other fluids (hydroperione), which may accumulate above it to an injurious extent; and there is certainly no reason to think that its displacement should involve the ovum in any danger. It is well known that the introduction of a probe into the uterus is not absolutely inconsistent with the continuance of pregnancy; and the same might be said of the introduction and maintenance there of an intrauterine pessary. My dissections of virgin and unimpregnated uteri have frequently, indeed generally, demonstrated the existence of this firm and adherent plug of mucus in the non-menstrual state. From these considerations, it is manifest that the secretion of the menstrual fluid from its ordinary source, and its subsequent discharge, are in no manner impossible in early pregnancy.

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* *Novi Comment. Acad. Scient. Imp. Petropol.* Tom. i. p. 349 : 1750.

† *Von dem Mangel*, etc., *der Gebärmutter*, p. 287.

It is known that, during pregnancy, maturation of Graafian vesicles and the discharge of ovula do not generally take place. But the occasional occurrence, in early pregnancy, of menstruation, with all its ordinary symptoms, suggests the probability that at such times perfect ovulation may take place. Should this happen, we have shown that there is no anatomical reason, during the first three months of pregnancy, why the ordinary menstrual flux should not proceed from its ordinary source, the mucous membrane of the body of the uterus now altered into the decidua vera of early pregnancy.

"That menstrual congestion," says Schultze, "often occurs during pregnancy is undoubted; menstrual bleeding also takes place; that a ripe ovum may reach the decidua can scarcely be denied, nor can the possibility of its being impregnated there."* But in spite of this opinion being held by perhaps the majority of physiologists, it cannot be held as proved, otherwise than by the occurrence of superfœtation. Those who deny the occurrence of superfœtation may deny also even the occasional occurrence of healthy ovulation during pregnancy. Kreuzer † relates a case which might have afforded evidence in favour of the possibility of ovulation during pregnancy, but it did not. He made the autopsy of a woman who had arrived at the fourth month of pregnancy, and who had appeared to menstruate thrice while in this condition. Only one corpus luteum was found in the ovaries, and no trace of a recently burst Graafian follicle.

Complete fusion of the decidua vera and decidua reflexa is generally described as taking place about the end of the third month of pregnancy. Menstruation proceeding from its usual source after this time, though not impossible, must be regarded as necessarily producing disruption of tissues, as rudely separating the coalesced layers, and as almost certainly leading to abortion, of which, in such cases, it would probably be regarded as merely the first symptom. "The intimate adherence," says Robin, "of the uterine and the reflected caduca, strongly pressed, the one against the other, in consequence of the increase of volume of the ovum, has, as a result, to bring into contact epithelial cells in the very thickness of the two caducæ, as they are mutually united, which at the time of labour appear to form only one layer, so intimate is the fusion of the two portions called uterine and reflected. Now this condition of the epithelial cells is not observed before about the fourth month, and might appear pathological to any one who had not followed the phases of the gradual, and at last intimate, cohesion of the uterine and reflected caducæ."*

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* *Jenaische Zeitung*, 1865; and *Biennial Retrospect of Medicine and Surgery*, 1865-6, p. 394.

† See Kussmaul, *Mangel*, etc., *der Gebärmutter*, S. 278.

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* *Mem. de l'Acad. Imp. de Médecine.* Tome xxv. p. 142.

CHAPTER II.

SUPERFŒTATION.

MENSTRUATION proper is a generally-received indication that conception is possible. And there is no valid reason to believe that this does not hold equally good of the menstruation occurring in early pregnancy as at other times.[*] Physiologists have found no difficulty in admitting the possibility of superfœtation during the first few days after conception, or as Velpeau,[†] Allen Thomson,[‡] and others, specially point out, till the uterus, as they suppose, becomes closed up by the decidua. Of this, the authentic instances of twin-births, where one child has been born black and the other white, in accordance with successive impregnations by black and white males, form incontrovertible illustrations. But the dissection above described (p. 163), and others, show that the uterus is not closed up till some time about the third or fourth month, and there is no reason to think superfœtation impossible till this takes place. It is now established, chiefly by the researches of Pouchet, Raciborski, and Bischoff, that a mature ovum is ordinarily expelled during or after every menstrual period. On the occurrence of pregnancy, the maturation of ova in the Graafian vesicles, and their expulsion, are generally arrested till near the end of the period of suckling. But this is not invariably the case. In the human female, the sign of ovulation being about to take place is the occurrence of menstruation. The discharge of the menstrual fluid necessarily displaces the mucous plug in the cervix of the pregnant as of the unimpregnated uterus, and thus clears a passage for the conjunction of the ovum of the female with the spermatozoa of the male.

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[*] For a full discussion of this point, see a paper by Scanzoni, entitled *Ueber die Fortdauer der Ovulation während der Schwangerschaft. Beiträge zur Geburtskunde und Gynaekologie*, IV. Band. S. 311, 1860.

[†] *Traité Complet des Accouch.* Bruxelles, 1835, p. 194.

[‡] *Cyclop. of Anatomy and Physiology*, vol. ii. p. 49.

The uterus does not become closed, and repeated impregnation is not impossible, till the ovum is so large as to force the decidua reflexa into close and firm apposition to the decidua vera. Up till this time there is a sufficiently free communication between the ovary and the vagina, or between the ova and the semen. In the uterus described, there is at least as much free space for this communication as exists in the long and very contracted cervical passage of the virgin uterus.

We have already quoted Weitbrecht's opinion as to the impossibility of superfœtation, in discussing this subject in the previous chapter. Mason Good says, that from the time of conception, "the cervix of the uterus is closed in its canal toward the upper part, so as to prevent a second fœtation by the introduction of fresh male semen."* In his work on Physiology,† Wagner makes this remark, "Once the decidua is formed, and the ovum has reached the uterus, fruitful intercourse is no longer possible, and the cases of superfœtation which have been admitted under these circumstances are physiological impossibilities; so that the recognition of such a circumstance ought to be banished from midwifery and legal medicine."

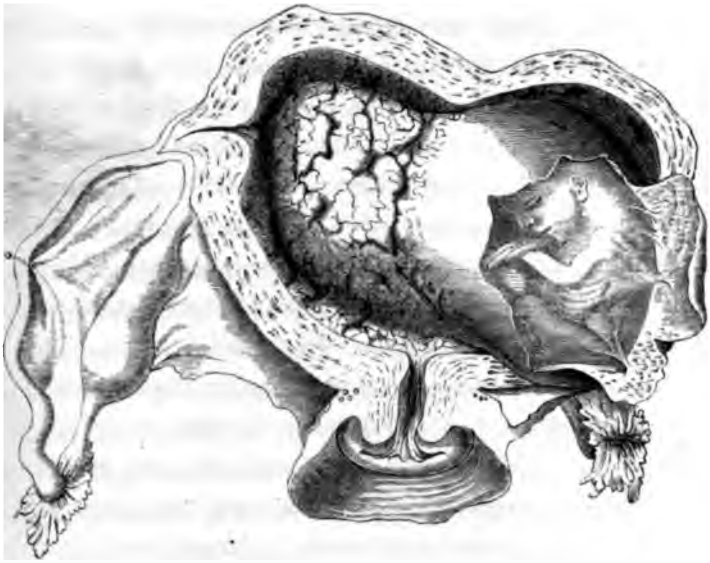

Fig. 19.

If Wagner had studied the plates of William Hunter, he would not have made this boldly erroneous statement. To illustrate its error, and to show the

Fig. 19. Reduced greatly from the original.

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* *Study of Medicine.* 1822. Vol. iv. p. 24. † Engl. Trans. p. 79.

possibility of superfœtation, so far as mechanical arrangements are concerned, I here only refer again to Coste's drawing of a third month pregnancy (Fig. 19). In it the free passage of communication between the internal os uteri and the Fallopian tube is seen, and it is only needful to suppose that an ovum was matured, to be supplied with the principal, if not all, the conditions necessary for a second conception and a double pregnancy.

Numerous physiologists adduce the plug of viscid cervical mucus as an impediment to impregnation during pregnancy; but frequent examinations have convinced me that this plug is dense and well developed, and apparently impassable in the non-menstruating unimpregnated uterus, and as it does not prevent conception then, so there is no reason to ascribe to it this function in early pregnancy. On this point Schultze remarks, "The cervical mucus is no obstacle, and the route from the vagina to the Fallopian tubes is open until the decidua reflexa is consolidated with the decidua vera; this happens only after twelve weeks, so that, as Matthews Duncan has already shown, there is possibility of superfœtation in the first three months of gestation."*

"Duncan," says Kussmaul,† referring to my original paper in the *Monthly Journal of Medical Science,* "defends the view, that even to the end of the eighth week of pregnancy, and therefore to the end of the second, and even to the beginning of the third menstrual period, superfœtation may take place. As he denies the impenetrability of the mucous plug, and holds as demonstrated the continuance of ovulation, he appeals to the experience of Coste and others, that till the eighth week of pregnancy a space is to be found between the decidua and the reflexa filled only with water, and therefore permeable by semen; and that as a rule the internal extremity of one Fallopian tube, as well as the internal os uteri, are open. In truth, it must be conceded to him, that up till the period named the ovum does not so completely fill the cavity of the uterus as to render impossible the penetration of the fructifying fluid as far as the tubes. Cases of pregnancy within and outside the uterus may be with equal propriety appealed to, in which, in spite of large fibroids and polypous swellings in the interior of the uterus, conception took place, in order to prove incontrovertibly, on the one hand, how complete must be the repletion of the uterine cavity to produce obstruction to the passage of semen into the tubes; on the other hand, to show that active secretion from the interior of the uterus, as must be in cases of polypi, does not always render impregnation impossible."*

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* *Jenaische Zeitung,* 1865; and *Biennial Retrospect of Medicine and Surgery,* 1865-6, p. 393.

† *Von dem Mangel,* etc., *der Geb.* S. 289.

The only insurmountable anatomical obstacle is the insertion of the ovum over the os uteri internum, or simultaneously over the internal orifices of both Fallopian tubes.

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* I may mention here that I believe I have seen a case of impregnation and continuing pregnancy in a woman wearing an intrauterine stem metallic pessary.

It may be supposed by some physiologists, who attach much importance in the function of fecundation to the ciliary movements, that their absence will have the effect of preventing the advance of the spermatozoa to meet the mature descending ovule. The researches of Robin * and others have shown that soon after conception the ciliated epithelium of the uterus is supplanted by a pavement epithelium without any cilia. But I have never seen reason to attach much importance to these movements in this function. Indeed, the directions of the ciliary currents in the human female are only arrived at by reasoning from analogy. Assuming, however, that they are as described, we find that the resistance offered by the ciliary motions in the tubes to the advance upwards of the spermatozoa, does not prevent them arriving in abundance at the ovaries. Perhaps, like salmon seeking the spawning beds, the spermatozoa are stimulated to activity by the opposing currents. My own impression is that the semen does not owe much of its progress to ciliary movements. At all events, the three following considerations prevent much importance being attached to such ciliary currents:—*First,* The rapidity and copiousness of the advance of the semen, in degree such as can scarcely be due to ciliary movements, as witnessed in experiments on animals; *Second,* The advance upwards through the tubes in which the ciliary movements are believed to be opposed in direction to those of the spermatozoa; *Third,* The fact that, after delivery, women sometimes are impregnated again before the reproduction of the ciliated epithelium of the uterus. I have known a living mature child born some days before ten calendar months had elapsed since the last confinement; and many interesting details of similar events may be found in a paper* by Dr. Bonnar of Cupar. "Counting from the day of confinement," says Robin,† "it takes about sixty-four or seventy days for the mucous membrane to complete its regeneration. . . . It is not till after the twentieth or twenty-fifth day, or even later, that epithelium, with cells of polyhedral shape, forms a continuous superficial layer, or nearly so, on the surface of the tissue. As time goes on it becomes more and more marked; but the epithelium does not become prismatic till very late, for, in a woman dead forty-two days after confinement, the epithelium was still formed of polyhedric cells." It may then be held that many a woman has conceived whose uterus presented no ciliated epithelial surface.

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* *Mem. et Comptes Rendus de la Soc. de Biologie*, 1855, p. 113; and *Mem. de l'Acad. Imp. de Med.* Tome xxv. 1861, p. 139.

Without going over the numerous cases of superfœtation which are everywhere recorded, my belief may be stated that this explanation will account for all the authentic cases. For if we suppose in an instance of this kind, that the first child is born prematurely, but within the limits of viability, we thus gain two months; and if impregnation may take place between two and three months after conception, we have thus four or five months of interval accounted for between the births of successive viable infants.

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* *Edinburgh Medical Journal.* January 1865.

† *Mem. de l'Acad. Imp. de Med.* tome xxv. 1861, p. 150.

In the same way it is easy to explain the difference in apparent age and development often observed in twin-births; which may thus be instances not unfrequently of superfœtation, with this peculiarity, that both children, though conceived at different times, are born at or near the same time. But I must add that all such cases cannot be explained by the supposition of superfœtation. Most conclusive evidence to this effect is afforded by a preparation in my collection, of a double monster (anterior duplicity), in which the fœtuses differ considerably in size; and it is difficult to imagine that superfœtation has anything to do with the anomaly of double monstrosity.* The united children had only arrived at about the end of the second month of gestation. In his observations on this point, Professor Späth appears to have misapprehended the purport of my remarks, which is identical with that of his own. I quote them on account of their importance. "These results," he says, "sufficiently establish the following positions:—1. That twins, in cases where they are developed from two ova, do not present more considerable differences in size than in cases where they have been developed from one and the same ovum. There is here, therefore, no good reason to regard superfecundation as the cause of difference of size in twins, although at the same time the possibility of such a cause is not excluded. 2. That in twins developed from the same ovum, and in spite of their intimate relation to one another, the difference in size may be as considerable as in single children, which may present a difference equivalent to two or three months' growth."

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* See a closely analogous case of Meissner's, mentioned by Schultze. —*Biennial Retrospect of Medicine and Surgery*, 1865-6, p. 394.

"Were we even to assume with Matthews Duncan the possibility of a second conception in the eighth or ninth week of pregnancy (an interval of time which I consider too long), we should not, according to the above data, be in a position to prove it by the fact of a difference in the size of the twins. Still less could we from the same facts deduce a proof of superfecundation; and I think that, in the difference in the size of twins—at least in the later periods of pregnancy generally—no proof for or against second conceptions can be looked for, as, according to the facts I have recorded, the size of twins developed from the same ovum may differ so considerably. Perhaps the examination of twin ova during the first months of pregnancy might teach something with regard to this question."* This statement of conclusions, from a large collection of facts, is very valuable; but it throws no light on the mystery,—Why does one fœtus grow more than its neighbour? And it appears to me, that from the phenomena of the arrested development of the ovum of the roe, in early pregnancy, nothing can be borrowed to illustrate it.

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* *Zeitschrift der K. K. Gesellsch. der Aerzte zu Wien;* and *Edinburgh Medical Journal*, March 1862, p. 849. See also Kussmaul, *Von dem Mangel*, etc. S. 295.

CHAPTER III.

THE SITE OF INSERTION OF THE OVUM.

WHEN first I wrote some passages on this subject, the work of Kussmaul, to which I have already often referred, was not published. This work contains a valuable chapter on the "Ueberwanderung des Eies," to which I have much pleasure in referring readers who wish to enter farther on the matter than I now propose to do.

The dissections already described, and numerous others, have demonstrated that the decidua is formed by the development of the normal mucous membrane of the uterus, without necessarily closing the tubes or cervix uteri. The only (and that not essentially) new production is the comparatively thin membrane of the decidua reflexa, which springs from the mucous membrane and envelopes the ovum. Burns,* Velpeau,† Campbell,‡ and others, with their erroneous notions of the origin of the decidua reflexa, have ascribed to it the function of limiting the motion of the ovum on its arrival in the uterus; and in the same way have attempted to account for the various positions of the placental insertion. But such opinions are now intenible.

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* *Anatomy of the Gravid Uterus*, p. 201.

† *Traité Complet des Accouch*. p. 158, etc.

‡ *System of Midwifery*, p. 85.

Although their anatomical knowledge was erroneous and misleading, some authors attained to what appears to me very accurate views on this subject. I quote from the works of Osiander. "In the uterus," says he, "of a primipara, and in the uterus of a married person who has had sufficient time to make a complete recovery from a previous pregnancy, the ovum, on emerging from the Fallopian tube, will find little space in the uterus, and will be under the necessity of remaining in the place where it entered. Consequently the little egg must take root in the neighbourhood of the tube on the fundus or side of the uterus. And therefore the placenta will, in a primipara, rarely be found elsewhere than on the fundus or on the side high up near the fundus; very rarely, or rather never, on the os uteri; sometimes, indeed, low down, not very far removed from the mouth of the womb when the pregnancy begins, shortly before, during, or after the monthly flow, as then the ovulum meets with more room in the uterus as it is relaxed at this time."

. . . . .

"It is quite otherwise with respect to the insertion of the ovum in multiparæ, and especially in persons whose womb is either naturally weak or has suffered in any way and lost its power: as through very great monthly cleansing, through leucorrhœa before or during married life, through many and difficult labours,

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takes root, as it were, somewhere in the vicinity of its point of ingress.*

The ovum, on its arrival in the uterus, will move in the direction of least resistance till it becomes fixed. Obstetricians have hitherto often been puzzled to explain the insertion of the ovum over the os uteri, or on the side of the uterus opposite to that indicated by the corpus luteum as the side by which it entered, and have suggested to themselves wonderful processes by which the ovum might burrow beneath the decidua to arrive at the distant site, or be otherwise conveyed thither. But our improved knowledge of the decidua dispels these difficulties. It may thus be easily conceived that if, on the arrival of the impregnated ovum in the uterus, the peculiar hypertrophy of its lining membrane exist only in a small degree, the ovum may enjoy some freedom of motion in the enlarged cavity. Such a state of matters might, *a priori*, be looked for in women having the peculiar erethism of menstruation in a low degree, or in such as have borne families, or in whom, from any cause, the cavity of the womb is enlarged.

At an advanced stage of pregnancy, our only guide as to the original site of the ovum is the insertion of the placenta. This may be found in any part of the uterine cavity; but its rarest and undoubtedly its most dangerous position, is when it is implanted over the cervix. Interesting dissections of the early occurrence of this state are to be found recorded by W. Hunter, in his 34th plate, and by Dr. R. Lee.*

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* These views are not at all inconsistent with those of Kussmaul, as he seems to think. *Ibid.* p. 322.

Now it is curiously in accordance with the details just given that we find the ovum descending to this position much more frequently in cases of repeated than of first pregnancies. Sir J. Y. Simpson has pointed out this fact, and suggests that it might afford some clue to the discovery of the cause or causes of placenta prævia. He has shown that, in the Dublin Hospital, while Dr. Collins was master, first labours formed thirty per cent of all the deliveries; and that notwithstanding this large proportion, not one of eleven cases of placenta prævia occurring in that time was a first pregnancy. Of 136 cases of placenta prævia tabulated by Dr. Simpson, only eleven were first labours. In fact, a very great majority of the women had had several children previously; and it is to be observed that these figures give far too low an estimate of the relative rarity of placenta prævia in first labours, on account of the great numerical excess of first over second or third labours, and so on.

I may here venture to offer the conjecture that it may be possible, with the aid of superfœtation, to explain the frequency of twins in pregnancies of high number, on the same grounds as are offered now in explanation of the occurrence of placenta prævia.

It may happen, in some cases, that from similar causes an impregnated ovum may not find an appropriate nidus for its development, either from the comparatively atrophied state of the decidual membrane, or from the patency of the uterine cavity admitting of its escape into the cervix below the seat of the proper decidual structure.

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* *British and Foreign Medico-Chirurgical Review*, July 1854, p. 27.

CHAPTER IV.

THE INTERNAL SURFACE OF THE UTERUS AFTER DELIVERY.

No greater step has in modern times been made in the department of obstetrical physiology than the establishment of the true anatomy of the decidua vera and decidua reflexa, and the light shed by these discoveries on various natural and morbid functions and processes has led to a great extension of obstetrical knowledge. The unsatisfactory nature of the old erroneous theories of John Hunter regarding the decidual membranes, is quite equalled by the unsatisfactory and unscientific doctrines generally taught regarding the same parts after labour or miscarriage. Although it appears to me that the unreasonableness of these doctrines, and their inconsistence with facts, have been well demonstrated, and that satisfactory though incomplete descriptions of the true anatomy of the mucous membrane of the uterus after delivery have been substituted for the old errors; yet it is evident, from an extensive acquaintance with obstetrical literature, that the old errors still flourish in the schools. Many authors, indeed, seem quite insensible of the immense importance of the subject. Others, among whom I may cite the justly eminent and highly enlightened Baron

Dubois of Paris, still appear to cling to the old doctrine connected with the name of Cruveilhier, that after delivery the muscular tissue of the body of the uterus is everywhere laid bare. In describing the internal plane of muscular fibres of the uterus after delivery, Dubois makes the following remarks :*—"Nous avons cherché à distinguer sur la coupe même de l'organe, de trois à sept jours après l'accouchement, les traces de la muqueuse en voie de régénération, nous n'avons jamais pu les distinguer. . . . La couche musculaire existe immédiatement au-dessous de la masse sanieuse d'un gris brunâtre, qui après la coction, remplit constamment l'organe chez les nouvelles accouchées."†

The internal surface of the uterus after delivery may be divided into three principal parts: 1. The inner surface of the cervix; 2. The site of the insertion of the placenta upon the inner surface of the body of the uterus; and 3. The rest of the inner surface of the body of the organ.

The first of these parts is entirely excluded from discussion in the present paper. The cervix of the uterus is now known to differ so materially in its anatomy and physiology from its body, that it may with propriety be described as almost a separate organ. Researches renewed by M. Stoltz have been continued by numerous authors, and show that the cervix uteri does not become developed so as to form part of the cavity of the organ in the way that it was formerly believed to do. More recent researches by anatomists and obstetricians, too numerous to be mentioned, have shown the entire difference, anatomically and structurally, as well as functionally, of the mucous membrane of these two parts. And, in more immediate relation to our present subject, it may be stated that there is little variance of opinion as to the inner surface of the cervix being provided with a mucous membrane after as well as before parturition.

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* *Traité Complet de l'Art des Accouchements*, par MM. Dubois et Pajot. Tome i. livr. 2me, p. 430.

† Helié also coincides with Dubois. *Fibres Musculaires de l'Uterus*, 1864, p. 30.

The other two parts of the internal surface of the uterus after delivery demand careful description.

Every attentive student of obstetrics must have been struck with the manifest paradoxes implied in the history commonly given of the termination of parturition, and the restoration of the inner surface of the uterus to its pristine condition. Obstetricians teach that with the expelled ovum there generally passes away the entire decidua, leaving the muscular tissue of the uterus bare over its whole inner surface; and, in contradiction at once to observation and analogy, describe the formation over this surface of a false membrane, and afterwards of a new mucous membrane —all this process being, even in the healthy female, an inflammatory one, and, like superficial inflammations in such structures, accompanied by the secretion of a quantity of pus which they describe as constituting the essential part of the lochia. To adduce evidence of this statement here is, perhaps, not necessary, as the description must be readily recognised by almost every one; and in a subsequent place I shall return to these errors, and dwell at some length upon them; at present I may merely remark, that it appears wonderful that so many physiological paradoxes should have been so easily adopted by the profession, and that so much of what is essentially morbid should have been invoked to aid in the performance of a natural function in a healthy body.

In the early months of pregnancy, the mucous membrane of the cavity of the uterus is very highly developed, forming a rich, soft lining to its inner surface, and contributing greatly to the increased thickness of the parietes of the organ at this time. When the walls of the uterus are examined in advanced pregnancy, the thickness of this structure is found to be greatly diminished, even when the whole structures between the chorion and the muscular tissue of the organ—that is, the proper mucous membrane, or decidua vera, and in addition, the decidua reflexa—are included in the measurement. If, in a woman who has died in advanced pregnancy, the membranes are separated from the uterus, mucous membrane is left adhering to the walls of the womb; and its surface is nearly smooth, at least in the expanded condition of the organ.* I have witnessed the *post-mortem* examination of a woman dying during labour and before rupture of the membranes, where the ovum was almost completely separated by hæmorrhage extravasated between the membranes and uterus. In this case the uterine fibres did not appear to be anywhere denuded; but the examination was not sufficiently minute and satisfactory.

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* These facts I have verified by means of several dissections preserved in the late Dr. Campbell's museum, and by others of my own. Albinus, in his *Atlas of Anatomical Plates,* describes the same results of a similar dissection. See *Uteri Gravidi,* Tab. vii. His words of description are " Ovum exemtum . . . . . Uteri pars interior, mollis,

In parturition a similar process takes place, with this difference, that after the removal or expulsion of the ovum, the uterus is reduced by its contractions to dimensions very small compared to those it had whilst expanded. The result of this contraction, upon the mucous membrane, resembles its effect on the muscular tissue of the organ. In both, the diminution of superficial extent is the result partly of the expulsion of the large mass of blood contained in their very large vessels, but chiefly of the assumption of a greatly increased thickness of wall. Immediately after parturition, the mucous membrane of the uterus is rough and irregular on the surface, and covered over with blood and adherent coagula.* In a preparation in my

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tenera, veluti spongiosa, canosaque: cui ovum molliter adhæret, involucro suo membranaceo." See also W. Hunter, *Anatomical Description of the Gravid Uterus*, Ed. 1843. With his admirable accuracy, W. Hunter describes (p. 47) the adhesion of the decidua to the muscular fibres of the uterus as being "rather stronger than the adhesion between its external (decidua vera) and internal stratum (decidua reflexa), which we may presume is the reason that in labour it so commonly leaves a stratum upon the inside of the uterus." And he elsewhere makes the general statement, that "one stratum of the decidua is always left upon the uterus after delivery." See also J. F. Meckel, *Descriptive Anatomy* (Eng. Trans.), vol. ii. p. 596.

* In his researches *On the most important Diseases of Women*, p. 36,

possession, taken from a woman who died soon after delivery, where the os uteri is still largely dilated, and the utero-placental sinuses open, the length of the uterus is about seven or eight inches: its thickness, including all its parts, is less than one inch. Its internal surface is covered by a thin membrane. This latter is thicker and more prominent at the seat of the placental insertion. Judging from the extent of surface occupied by open-mouthed veins, the placental site is reduced to a circular or rather oblong space, of about three inches in diameter. In another preparation, the uterus is of about the same size, above an inch in thickness: the site of the placenta is evident from the prominence and softness of the part, the uterine sinuses not prominent and gaping, but apparently closed, and the mucous lining thicker and soft. This uterus is evidently that of a woman who has died some time after delivery. In another case, where the mother died on the fourth day after delivery, I had, through the kindness of Dr. W. T. Gairdner, an opportunity of

Dr. Robert Lee thus describes the interior of the uterus after labour in the healthy state: "For several days after delivery, where no disease of the uterus has supervened, its lining membrane is coated with a yellowish-brown, dark-red, or ash-grey coloured layer, of no great thickness, which seems to be formed chiefly of the fibrine of the blood with small portions of deciduous membrane . . . . Where the placenta had adhered, numerous dark-coloured coagula of blood are found to seal up the orifices of the uterine sinuses in the inner membrane, and frequently to extend a considerable distance into these veins. The clots of blood, one extremity of which hangs loose within the cavity of the uterus, are often connected with a large fibrinous coagulum, which entirely fills the fundus uteri, and everywhere firmly adheres to the inner surface of the organ."

examining the uterus. It measured about seven inches in its greatest length, and the thickness of its walls was about three-quarters of an inch. The whole inner surface of the organ was manifestly covered by a mucous membrane; lacerated at the site of the placental insertion, a surface of between three and four inches in diameter, a number of clots of blood being entangled in the venous openings. Elsewhere, the mucous membrane was distinct. It was covered by the lochial secretion. On scraping the surface, the lochia and epithelium were easily removed, laying bare the fibro-cellular structures of the mucous membrane beneath. In another preparation, which I examined carefully with Dr. W. T. Gairdner, and procured from a woman who, although engaged nursing a child, denied having been recently delivered, and where the woman died of phthisis, we found the lining membrane of the uterus very thick, forming nearly one-third of the entire thickness of the walls of the organ, but thinning rapidly as it approached the cervix. The site of placental insertion was still marked by the prominence of the lining membrane, by its numerous elevations and depressions, covered with sanious fluid, and by the large sinuses, which were easily seen in a cross section, proceeding through the mucous tissue to open on the very surface of the membrane—making it evident that the mucous membrane is not, as Heschl describes, developed as a new production over the venous orifices. Between the placental insertion and the cervix, a punctated appearance, produced apparently by the openings of the reappearing follicles of the membrane, was seen. In this case the uterus measured about five inches in length, and the woman had passed the period of confinement at least four weeks. In the uterus of a woman dying the day after a difficult delivery, I found its inner surface covered by a copious soft membrane, the inner surface of which was very dark in colour, covered with blood, and almost gangrenous in appearance. The woman died from peritonitis, the result of hæmorrhage into the peritoneal cavity from a rupture. In addition, I may state that all authors, even those who assert that the muscular fibres of the uterus are denuded after delivery, as Cruveilhier, Fergusson, and others, yet describe their dissections as displaying an inner or lining membrane covering the inside of the uterus.*

In a paper by Dr. Heschl, published in the second volume, for 1852, of the *Journal of the Imperial Royal Society of Physicians of Vienna* (p. 228), and translated into English by Dr. M'Donnell of Dublin, and entitled "Researches on the Conduct of the Human Uterus after Delivery," the subject of the state of the internal surface of the uterus after delivery is only casually entered upon, as the main object of the paper is to describe the changes taking place in the muscular fibres of this organ. About the commencement of his paper the following passage occurs:

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* See the cases recorded in Livraison, xiii. of Cruveilhier's *Atlas of Pathological Anatomy;* also the cases in Dr. Fergusson's *Essay on Puerperal Fever.*

—"The placenta-spot, which always occupies a third part of the inner surface of the contracted uterus, still retains a projecting, uneven, and considerably lacerated surface. The rest of the inner surface of the corpus uteri is composed of the bare muscular substance, from which hang here and there shreds, the remains of the decidua. The mucous membrane of the cervix is in the great majority of cases quite complete, with hypertrophied papillæ, glands, etc., and is covered with a good deal of jelly-like mucus: in some cases it is deficient in some places, or up to the os externum."* To these remarks I do not attach much importance, seeing that they are probably a statement of the usually entertained notions, without having subjected them to careful consideration and the criticism of microscopical observation. In his compendium of general and special pathological anatomy, the same author, Heschl, makes similar statements, and almost developes a theory of the supposed regeneration of the mucous membrane of this surface.† It takes place, he says, through an exudation beginning after delivery, and covering the surface, like a thin layer of a grey-red juice, and possessing on the third day after delivery a scaly epithelium. He adds to this description, that in abnormal puerperal conditions (*puerperalen processen*) the inner surface of the uterus may be covered with a layer of pus or fibrin, often containing blood, and of a yellow or yellow-green colour.

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* See M'Donnell's Translation, p. 8.

† *Compendium der allgemeinen und speciellen Pathologischen Anatomie.* Von Richard Heschl, Doctor der Medicin, etc. Wien, 1855. S. 437.

Towards the end of his paper first referred to, Heschl makes the following more detailed remarks on this subject:—" A couple of days after the birth, the entire inner surface of the uterus appears covered with a more or less red-coloured, soft, pap-like, flaky substance. If the mucous membrane of the cervix uteri continues to exist, then from its margin, or, if not, from the exterior of the os, this substance, in the form of a very fine-meshed net, is seen to spread over the inner surface of the uterus in layers, the thickness of a sheet of paper; whilst this network progresses, the under meshes by degrees become narrower and fall together, and the fretted inner surface of the uterus becomes everywhere covered with it. On microscopic examination, it appears that this pap, at first pale, consists of pavement and cylindrical epithelium (also of mica-like cells ?) whilst in the deeper layers, lying immediately on the muscular substance, it consists of young cellular tissue."*

These observations of Heschl make his views easily understood, and I shall only venture to make one or two remarks on them. First, he adduces no evidence, founded on actual examination, that the muscular uterine fibres are, as he asserts, bare after delivery. Secondly, the earliest microscopical evidence adduced by him is the examination of an uterus four days after delivery (see his plate, Fig. 5), in which the inner

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* M'Donnell's Translation, p. 13.

uterine mucous membrane is carried away along with it."

These brief and casual statements of Virchow are of great value, on account of the weight afforded them by the author's name. They entirely coincide with views and statements previously made by me in the Physiological Society of Edinburgh. It will be observed that in two cases, the only ones whose actual examination is recorded, the whole decidua vera was, in Virchow's opinion, left upon the uterine surface. But this condition he evidently does not regard as the ordinary state after delivery. The common condition, according to him, consists in the body of the uterus being covered by the remains only of its decidua vera or mucous membrane, whose inner or superficial parts are carried away with the membranes in course of delivery. In regard to the condition of the placental site, Virchow's views are not at all distinct, seeing that it is unjustifiable to guess to what extent he would wish the word "wounding" (*verwundung*) to indicate a meaning different from that of the condition of the rest of the uterine surface, the internal layer of whose mucous membrane he regards as torn or carried away (*abgerissen*).

In the *Monthly Journal of Medical Science* for September 1854, Dr. Chisholm published a paper entitled "Two Cases in Midwifery, with a Description of the Internal Surface of the Uterus after delivery." His observations, regarding this last subject, were made in the case of a woman dying on the seventh day after delivery, and are restricted to the general surface of the organ, excluding the placental site. He describes the general internal surface as "covered with a thick tenacious mucus. This, when examined microscopically, was found to contain much molecular and granular matter, a few pus corpuscles, and about as many red corpuscles. There was also a considerable proportion of nucleated cells, variable in size and shape, but all more or less closely resembling the normal epithelial cells of the uterus; these were most numerous when a scraping was taken from the internal membrane with some degree of force. None of the muscular fibres could be obtained by scraping, unless the tissue were torn up. The internal surface itself was of a pale rose colour; when examined with the naked eye or low powers of the microscope it seemed quite smooth, and this smoothness was equally apparent when it was examined under water; it did not present that ragged aspect we must have found if the muscular tissue had been laid bare. There were numbers of minute oval and circular depressions studded very regularly over the internal surface. These were distinctly visible to the naked eye, and the largest of them might have admitted the head of a small pin. Little more concerning these depressions could be made out with the microscope; but we have found similar appearances constantly presented by the mucous membranes of pregnant uteri among the lower mammalia, and no one can for a moment doubt that they are the openings of the follicles of the mucous membrane. One portion was dried, and when thin sections of this were placed under the microscope, it was found that a structure, assuming the appearance of a narrow clear line, existed internal to the muscular fibres. Another portion was macerated in water, and after it had lain three weeks a membrane could be moved separately upon the muscular tissue, and small flaky portions of it could be raised up with the forceps."

I may also refer to the magnificent work of M. Coste, entitled "Histoire générale et particuliere du développement des corps organisés." I cite this work merely to call attention to the tenth plate in the Atlas accompanying it. In this, the powers of artistic skill are exhausted in a beautiful rendering of the naked eye appearances of the uterus after delivery. These drawings are, in every particular, confirmatory of the views expressed in the preceding parts of this paper on the subject of the internal surface of the uterus after delivery, although M. Coste, in his description of them, evidently betrays an acquiescence in the old and erroneous notions so generally taught even now.

Dr. Priestley's opinions I shall give in his own words.

"The summary of conclusions may be stated as follows:—

"1. After an ordinary labour, terminating in separation and expulsion of the secundines, the muscular substance of the uterus is nowhere laid bare, as some authors have supposed, nor is there any inflammatory false membrane spread over the surface. As Dr.

Duncan has correctly pointed out, the interior of the uterus after parturition only bears analogy with the stump after amputation, inasmuch as both have large open vessels liable to be inflamed and to absorb noxious materials.

"2. When the membranes are thrown off in the third stage of labour, a portion of the decidua remains attached to the uterine surface as a protection against external agencies. It is not, however, strictly correct to assert that original mucous membrane may be found everywhere lining the interior of the uterus after delivery; this is true only with regard to the cervix. The mucous membrane of the body, and fundus of the womb, were transformed into decidual structures at the commencement of pregnancy; and the lamina of the decidua, found covering the surface of the interior of the uterus after delivery, has not the texture, nor can it fulfil the functions of a mucous membrane, but consists of an arrangement of cells, fibres, and fat granules, identical with those recognised in the decidua in the later periods of pregnancy.

"3. A new mucous membrane begins to be formed in the later months of pregnancy between the decidua and muscular coat. It undergoes a rapid development after the uterus is emptied of its contents; and as it is gradually perfected it assumes the function of the original mucous coat. During the process, the remains of the decidua undergo farther fatty change, and are slowly shed off to mingle with the lochial discharge.

"In cases where dysmenorrhœal casts and early ova are expelled from the uterus, the conversion of the mucous membrane into decidual structures is probably less complete, and the deep stratum remains behind to prevent the denudation of the muscular coat."*

Dr. Tanner in a very recent work has made a contribution to this topic. I here quote it, but at the same time I must add that I do not know what value to attach to it, for the author has given no anatomical or other reasons for the peculiar views he entertains. Yet I may express a hope that, in a special paper, he will allow himself full scope for the interesting discussion desiderated from him. His words are—"What really happens is probably this: about one month prior to parturition setting in an imperfect mucous membrane begins to be developed under the decidua vera; so that after labour, although the muscular fibres are quite exposed at the site of the insertion of the placenta, yet at other points of the body of the uterus there is a distinct semi-transparent film, which subsequently becomes developed into a perfect mucous membrane."†

It is then found that after delivery the muscular fibres of the uterus are not laid bare, but are covered by a mucous membrane. This membrane is undoubtedly the remains of the uterine decidua, a mucous structure having the peculiar characters of the uterine mucous membrane. When examined at this time it is found, as in early pregnancy, to be thicker at the site of the insertion of the placenta than elsewhere. In that site its surface is diversified by numerous elevations and depressions, and by the open mouths of uterine sinuses, which have been, as it were, cut across by the separation of the placental mass.

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* *Lectures on the Development of the Gravid Uterus*, p. 100. A thesis by M. Colin is mentioned by Priestley, and cited by Robin, as enunciating views identical with those expressed by me, but I have not been fortunate enough to see it.

† *Signs and Diseases of Pregnancy*, 2d edition, p. 100.

Since my first investigations were made, I have had opportunities of carefully examining the uterine surface at the placental site after natural labour, and in a suicide near the full time, in whom I artificially separated the organ with a view to this question. In some of these studies I had the valued assistance of the late Mr. Barlow, of this town, who, by microscopic examinations, made out the existence of structures such as have more recently been fully described by Priestley,* and especially by Robin. This last observer has now arrived at the same conclusions as I had previously announced. In his elaborate paper, published in the "Memoirs of the Imperial Academy of Medicine,"† he says, that of the utero-placental area of the mucous membrane of the uterus there is only a small superficial portion caducous, the greater part remaining permanently adherent to the uterus, and that this persisting portion has a thickness increasing in proportion to the contraction of the muscular layer of the organ.

M. Robin and Dr. Priestley believe that, in the course of pregnancy, after its fourth month has passed, the whole mucous membrane of the uterus, or the whole decidua, with the exception of the serotina, is thrown off from the subjacent muscular layer, being displaced by a new mucous lamina which at that time springs up beneath the old decidua. Up till 1858 Robin believed that a somewhat similar series of changes occurred after delivery at the site of the serotina or old site of the placenta, but now* he holds that the layer of serotina persisting after delivery is, in no sense, caducous, and continues attached to the muscular uterine wall.

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* Lectures on the Development of the Gravid Uterus, p. 97.

† Mémoires de l'Académie Impériale de Médecine, 1861, p. 137.

The part of M. Robin's views last described, as only recently adopted by him, coincides with what I have ever maintained. From all the other opinions of Robin and Priestley on this last point I dissent. Their published observations do not seem to me to afford any good grounds for their statements. Every one admits that a great amount of effete decidual structures is observed outside the chorion in advanced pregnancy, and it is only natural to suppose that, as superficial layers are shed or exfoliated, new structures are springing up to replace them. In this way it may be quite true that the old decidua is removed and a new mucous membrane appears. But in this sense the same may be asserted of the muscular layer of the uterus after delivery; the old has disappeared by absorption, and new muscular structures have replaced it; yet, after all, it is the same muscular layer. This is not the meaning of M. Robin and Dr. Priestley. They adhere to the old and, I believe, erroneous notion that the whole uterine mucous membrane of early pregnancy is deciduous or caducous, and is replaced by an entirely new membrane, the peculiar difference of their opinion from the older notions being this, namely, that while, according to the latter, it was at delivery that the decidua was torn off or thrown off, leaving the muscular structure denuded, according to the former it is about the middle of pregnancy that the decidua is separated and a new mucous membrane begins to be regenerated. This implies that at some time the muscular layer is laid bare, and produces on its surface a mucous tissue heterologous to it. On theoretical grounds this view appears to be intenible, and my observations in cases of miscarriage lend no support to it.

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* *Loc. cit.*, p. 137.

The cotyledonary placenta in the living cow, and in many other species of quadrupeds, is formed, like the human placenta, of an umbilical or fœtal portion, and an uterine or maternal portion. But the process of separation of the placenta in parturition is conducted very differently in these two animals. In the cow, the placental mass is not entirely separated from the uterus. The fœtal part alone is discharged with the membranes ; the maternal part is left attached to the mucous membrane of the uterus, of which it is merely a part highly developed for a special object. There is no wound left upon the surface of the uterus of the cow ; no vessels are divided in the process. But although there is no wound, yet the cotyledonary surfaces are left in a denuded condition, these parts not being covered by the ordinary epithelial structures. In woman, the separation of the placenta is very different. The after-birth of the human female consists of two parts—a fœtal and a maternal portion. The essential part of the maternal portion is the development of the inner tunic of the uterine vessels, which is prolonged into the after-birth, and springs from the numerous vascular canals which traverse the decidua at the site of the insertion of the placenta, and which are cut across, as it were, by the separation of the placenta. The after-birth of woman thus contains, besides the fœtal structures, the important maternal venous structure just mentioned; and also the layer of decidua which covers its surface, and is described as throwing processes into its substance. The separation of the placenta thus involves the cleaving of the mucous or decidual structure at the line of separation, and the section of the serous internal tunic of the vascular system of the mother at numerous points. And when we consider the extremely soft and fragile nature of both these structures, we shall not be astonished at the great facility of separating the placenta.*

After the separation of the afterbirth the internal surface of the contracted uterus at its former site is left covered by a thicker layer of the decidua than elsewhere. The internal surface is at this part more rough and irregular, and more prominent, than elsewhere.* The vascular openings are numerous, and spread over a surface which is, from the contraction of the entire organ, much smaller than the original surface occupied by the placenta.

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* "The separation," says W. Hunter, "of the placenta from the uterus, is commonly practicable with the least imaginable force."—*Anatomical Description of the Human Gravid Uterus*, ed. 1843, p. 37.

But, even in woman, the anatomical imitation of the process, as it takes place in the cow, is not impossible. W. Hunter succeeded in performing this operation in a conception of four months. At this time, he says, the union of the two constituent portions of the placenta is less intimate, and they may both be preserved very entire, like the vascular chorion and fungus in the quadruped.*

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* This prominence has often been mistaken for disease. Sometimes it is morbidly prominent, as is described in a subsequent part of this work on inverted uterus. See also some remarks by Professor Rolleston, who refers to Cazeaux.—*Medical Times and Gazette*, vol. ii. 1863, p. 337.

In another place Rolleston makes the following important remarks: "Owing to errors of observation, the name 'decidua serotina' has been applied to the structure I would call 'non-deciduous serotina,' as well as to that to which Dr. Priestley, like myself, would limit it. It is called 'parietal decidua' by Professor Goodsir, and 'caduque interutero-placentaire' by M. Robin. And in an account of it given by Professor Kölliker, I find him speaking of it in the human subject as "eine zusammenhangende Haut wenn er gut erhalten est.' It is sometimes called 'placenta materna;' but this phrase is applied to the maternal element of the 'placenta' also, and the adoption of it would consequently cause confusion. That the utero-placental area is, after parturition, covered by a layer of mucous tissue, and that the muscular coat is not laid bare at that period, but protected by a more or less consistent and coherent coating, to which I would affix the name of 'non-deciduous serotina,' was clearly shown in the year 1853 by Dr. Matthews Duncan, and has been subsequently confirmed by Drs. Chisholm and Priestley in Great Britain, and by M. Robin in France.

"This is not the place for histological and pathological details, such as will be found in the literature to which I have just referred; but, from a zoological point of view, it may be remarked that the fact of the non-regeneration of the uterine cotyledons of the ruminant, after accidental separation of them from the uterine wall, lends the strongest confirmation to Dr. Matthews Duncan's views. It has been most satisfactorily shown that, after such an occurrence, the place of the lost cotyledon is occupied, not by fresh mucous membrane, but merely by a white cicatrix."—*Transactions of the Zoological Society*, vol. v. p. 289.

The membrane is easily distinguished from the muscular tissue of the uterus by its softness, and by difference of colour in a cross section. It is in some cases so soft that its surface, and sometimes its entire thickness, can with facility be almost completely rubbed off or brushed off the subjacent tissue. And if this manipulation be practised upon it, the investigator will assuredly find no difficulty in discovering the muscular fibrous tissue to be bare, like the muscles in an amputated stump. This softness and friability is undoubtedly one of its characters which has given rise to the erroneous opinions of authors, for we frequently find them speaking of removing a soft membranous or flaky structure, in order, as they imagined, to display the real internal surface of the womb. For example, W. Hunter, in describing† a dissection of the uterus of a woman who died at the end of the ninth month without being in labour, states that, finding the internal surface of the uterus everywhere covered with a thin stratum of decidua, he rubbed off the tender membrane with a cloth, in order to expose the subjacent muscular structure. But numerous later investigators have not so correctly appreciated, as W. Hunter did, the nature of the structure they removed in a like manner, and which they believed to be effused lymph, false membrane, bloody coagula, or patches of decidua having no necessary existence there, and ready to be discharged or rubbed off in order to expose the muscular fibres, which they erroneously believed came to be exposed after parturition. We thus find that here, as elsewhere,[*] W. Hunter's accounts of his dissections are still in our day true in almost every particular. But whilst there can be no doubt of the truth of W. Hunter's anatomical description of this part, exception may very justly be taken to the opinion he expresses as to this residuary decidua. He states his belief that "most of it dissolves, and comes away with the lochia."[†] Now there is every probability that this takes place in a very different way. The residuary decidua forms the mucous

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* In regard to this dissection, Hunter adds, "I wished to give a figure of it, but the processes were so irregular and so changeable, while floating in the water, that the painter could not express them; and when taken out of the water they collapsed into a smooth membranous appearance."—*Anatomical Description of the Human Gravid Uterus*, 1843, p. 36.

† *Ibid.* p. 26. In his paper on the "Muscularity of the Uterus," Sir C. Bell writes thus: "Upon inverting the uterus, and brushing off the decidua, the muscular structure is very distinctly seen."—*London*

Medico-Chirurgical Transactions, vol. iv. p. 341. Speaking of a like proceeding, Virchow says, "Die dicke und sehr lockere Schleimhaut trennte sich von der Muskelwand ziemlich leicht, so jedoch, dass die tieferen Schichten auf der Muscularis sitzen bleiben."—*Abhandlungen*, etc. S. 781.

[*] See, in regard to the decidua in early pregnancy, *Monthly Journal of Medical Sciences* for April 1853, p. 326, or p. 161 of this volume.

[†] *Loc. cit.* p. 47.

lining of the uterus, and may pass away from the uterus, not in mass, but in the regular insensible exfoliation of such structures, or be removed by the vessels in its substance in the ordinary course of nutrition and absorption.

Moreover, when we inquire into the real nature of the discharges from the uterus after delivery, we shall find that they are of a nature and character quite antagonistic to the notions entertained as to the denudation of the muscular fibres, and the formation of a new mucous membrane after the deposition of a false membrane over the supposed wound, and under the influence of an inflammatory process. No doubt, the authors who hold these views state that, in accordance with them, the lochia are essentially a puriform fluid, at first mixed with blood, and afterwards giving place to a serous discharge;* but it is impossible to understand whence these authors have derived their information as to the purulent discharge which they describe the lochia to be.

To the most ordinary observer, the lochia in the healthy female always present an appearance far removed from that of purulent discharge from an extensive superficial wound, like that described to exist in the uterus at this time. The lochia have been frequently and correctly described as presenting three different appearances, in correspondence with three different stages, in the condition of the internal uterine surface after delivery. These have received the names of *lochia cruenta*, *lochia serosa*, and *lochia alba, vel mucosa, vel lactea*;[*] these last are sometimes also called *purulenta*, but this more from theoretical notions in regard to them than from any naked-eye resemblance they show to purulent discharge. No doubt, pus may often be observed in greater or less abundance in the lochia mucosa, proceeding probably from patches of inflamed surface on the uterus or vagina, or from healing lacerations; but laudable purulent discharge is not found in any quantity, if at all, in the healthy lochia. After the blood has disappeared from the lochia, they are generally observed to be a more or less clear viscid fluid, of a whitish, brownish, or yellowish colour, and wanting the rich yellow colour, creamy consistence, and appearance of pus discharged from a healthy wound. Under the microscope[†] they present blood-corpuscles, entire or breaking-up, epithelial cells of various forms, and abundant detritus of the same; and along with these, some mucus or pus globules. When pus, in any considerable proportion, becomes mixed with the lochia, as is not unfrequently the case, it is discovered by its different appearance and characters.

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* "Ordinarily," says Cruveilhier, "this false membrane is thrown off with a purulent discharge, which is the lochia."—See Fergusson, *loc. cit.* See also Velpeau, *Traité des Acc.* Ed. Bruxelles, p. 518; Litzmann, "Aufsatz über Schwangerschaft," *Wagner's Handwörterbuch*, iii. 2, § 135.

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[*] See Maygrier, *l'Art des Accouch.* tom ii. p. 218; Boivin, *Mém. de l'Art des Accouch.* p. 446; Chailly, *Traité Prat. des Acc.* 1853, p. 395; Jacquemier, *Manuel des Acc.* tom. ii. p. 584; Churchill, *Theory*, etc. *of Midwifery*, second edition, p. 188; Nægele, *Lehrbuch der Geb. für Heb.* p. 186; Scanzoni, *Lehrbuch der Geb.* p. 267.

[†] See Gruby, *Morphologia Pathologica*, p. 20, for some remarks on this subject.

Of late years[*] it has been customary to compare the internal surface of the body of the uterus after delivery to a great wound, or solution of continuity. This fallacious analogy was in modern times originated by Van Swieten; but it is due to him to state, that his notions on this subject were, in many respects, more true and correct than those of Cruveilhier and his followers in our own day. Thus, Van Swieten points out,[†] that after the separation of the chorion and placenta, the inside of the uterus is left covered with the remains of the *tunica cellulosa*, or *substantia cellulosa*, which separates the chorion from the proper tissue of the uterus. He does not describe the uterine muscular fibres as being laid bare, but states that, in addition to the rupture of numerous large vessels, there is[‡]—in his own words—*soluta cohæsio, recens, cruenta, partis mollis; id est vulnus;* and then he goes on to point out the particulars in which this solution of continuity differs from an ordinary wound. In a subsequent paragraph he states his belief that the fever, commonly called *febris lactea*, does not arise from the afflux of milk to the mammæ so much as from the superficial suppuration which he describes as taking place on the internal uterine surfaces.[§] In his treatise on childbed fevers, Dr. Kirkland[*] takes opportunity to corroborate the statements of Van Swieten on this subject.

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[*] Rigby, *System of Midwifery*, p. 269.

[†] *Commentaria in Aphorismos*, etc., Lugduni, 1764, tom. iv. p. 604, [1329]: "Cum ergo uteri superficies . . . laceræ tunicæ illius cellulosæ reliquias habet."

[‡] *Ibid.* p. 604.

[§] *Ibid.* p. 605: "*Videtur hinc valde probabile esse, febriculam illam,*

M. Cruveilhier is the anatomist of our own times who has been most explicit in this matter; and it is to his misstatements that are due many of the anatomical and pathological errors in regard to it which have found currency among distinguished teachers and authors in our own and other countries. Cruveilhier's views are detailed in his great work on pathological anatomy;[†] and his words are quoted with approval by Drs. Fergusson,[‡] Rigby,[§] and numerous others.

In the passage alluded to, one of the first statements, in regard to the inner surface of the uterus, is to the effect that the "whole of the mucous membrane has been altered by the inflammation of which it has been the seat."[‖] It is impossible to reconcile this quotation with what follows it almost immediately, to the effect that "except just at the inner surface of the cervix uteri, there is no mucous membrane at all; but

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quæ in puerperis lactea solet dici, non tantum a lacte ad mammas delato nasci, sed etiam a depuratione uteri per blandam talem, et superficiariam, suppurationem."

[*] *A Treatise on Childbed Fevers, and on the Method of Preventing them*, etc. etc., 1774, p. 75.

[†] Livraison xiii.

[‡] *Essays on the Most Important Diseases of Women*, part i. p. 77.

[§] *System of Midwifery*, p. 270. For similar opinions see Legallois, *Journal Hebdom. de Med.* 1829, tom. iii. p. 183.

[‖] *Essays on the Diseases of Women*, by Dr. Fergusson, p. 77. The words in the original are—"*La muqueuse intérieure n'existe plus ou plutot ses éléments se sont dissociés, modifiés par l'inflammation dont elle a été le siège.*"

the muscular tissue of the uterus is everywhere exposed."[*] It is quite needless to attempt to make these statements tally, seeing that in the act of parturition, and in the healthy puerperal state, no inflammatory process goes on whatever. Cruveilhier's assertion is totally unsupported by observation, and is quite inconsistent with the general principles of physiology, which do not admit of any morbid process, such as inflammation, acting a primary or subordinate part in any natural and healthy function. In natural parturition, as in all other healthy actions of the body, inflammation interpolates itself only as a cause of derangement or destruction of the function. Not very long ago, the history of the origin of the decidua, or modified mucous lining of the uterus, was involved in obscurity, and John Hunter, in attempting a theory for it, founded on erroneous and imperfect observations, was driven to suppose that in healthy pregnancy a sort of inflammation was set up on the internal surface of the uterus, producing there a layer of coagulable lymph, which he imagined came to form the decidua. Into a similar error M. Cruveilhier and his followers have fallen. There is no evidence whatever of the existence of inflammation of the internal surface of the uterus in or after natural delivery.

M. Cruveilhier goes on to state, that after parturition "the muscular fibres of the uterus are everywhere exposed."* But of this he gives no evidence whatever. On the contrary, the dissections he describes, although they are morbid, give some evidence to the contrary effect. In fact, the examination of this part after death overthrows this statement of Cruveilhier, Fergusson, and others. The actual observations of these authors form a curious commentary upon their expressed opinions. It is unnecessary to refer to and quote these observations, where the inner membrane of the uterus covering its muscular fibres is incidentally mentioned or described at length.

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[*] *Ibid.* p. 77. The words in the original are—"*On ne trouve de debris de muqueuse que sur la face interne du col uterin et quelquefois autour de l'orifice des trompes; partout ailleurs le tissu propre de l'uterus est à nu et partout il doit être recouvert d'une cicatrice.*" See also Heschl on the *Human Uterus after Delivery*, 1853, p. 6.

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* In a paper on the "Dysmenorrhœal Membrane," published in the *Monthly Journal of Medical Science* for September 1846, Sir J. Y. Simpson states his belief that the actual absence of the mucous surface of the uterus has been often ascertained on dissection, and adds, "I lately saw a case where the patient died six weeks after delivery, and still, at that late date after confinement, the mucous lining of the uterus was not yet regenerated." This case is not given at sufficient length to allow of any judgment in regard to it, and as it stands it is quite unsatisfactory. Further, Dr. Simpson states that "the absence of the mucous lining of the uterus in persons who have died after delivery, or who have been previously subject to membranous dysmenorrhœa, may have given rise to the strong opinions expressed . . . . in regard to the human uterus not being normally provided with a mucous membrane." But it has yet to be shown that at any time the uterus is denuded of mucous membrane. And certainly there is no reason to believe that, as Dr. Simpson states, "the proper mucous tissue of the uterus itself may, within the compass of a menstrual period, form, enlarge, separate, and again be reproduced ; and further, that all this may occur and continue regularly for a succession of months, or, as sometimes happens, for a succession of years." The discharge of the dysmenorrhœal membrane (or, as it may be called, inflamed menstrual decidua) does not involve the removal of any part of the entire thickness of the mucous membrane. A comparison will illustrate my meaning. The cuticle is, in the healthy state, constantly and insensibly exfoliating in dusty particles. In like manner the mucous membrane of the uterine cavity undergoes an insensible exfoliation. But let the skin be inflamed, as by a blister, and then the cuticle is detached in mass and separated as a membrane ; the proper tissue of the skin is, however, not removed. In like manner, when the mucous membrane of the uterine cavity is inflamed, as in dysmenorrhœa, and under other conditions, a thick mass is detached and separated as a membrane ; the proper mucous tissue of the cavity of the uterus is, however, not wholly removed.

Finally, did there exist after every delivery a wound of the enormous dimensions of the internal surface of the uterus—dimensions not inferior to those of the wound produced in amputation of the thigh, it is difficult to conceive how parturient females should escape the frightful mortality succeeding that operation, or the like. It would be difficult or impossible to explain why, instead of one in every three or four dying as after amputation of the thigh, there should be only one in every two or three hundred. It can scarcely be asserted that the shock produced, and the circumstances of the supposed uterine wound, are a whit more favourable to recovery in the obstetric than in the surgical patient. The explanation lies in the fact that the chief analogy of the internal uterine surface after delivery is not with a stump, so far as it consists of incised and denuded tissues, but only in both surfaces presenting numerous open veins liable to become inflamed, or to absorb the obnoxious materials which may be brought into contact with them. And it is to this anatomical circumstance that are traceable most of the cases of death in childbed.

CHAPTER V.

ON THE LOCHIA.

THE following are the results as to the constitution of the lochia which Wertheimer has deduced from observations made in eighteen cases of delivery.* I give them in his own words translated.

"From these individual observations it is easily to be seen that a distinct single picture of the physiological behaviour of the lochial secretion can be drawn up only by confining it to a few facts, especially if the duration of the secretion is considered.

"I shall try, in the following, so far as is possible, to bring together what is constant, and to state separately the less constant and accidental.

"I. Immediately after the end of labour the flow from the genital passages, often for several hours, even sometimes for a day, as I have sometimes observed, is purely bloody, with loose fibrinous clots.

"II. Thereupon there follows, or begins at once after the expulsion of the afterbirth, the exudation of a serous fluid, which always brings with it smaller or larger portions of vaginal mucus, which, mixed with blood-corpuscles, deposits itself in any vessel as a viscid stringy sediment. This serous fluid, according to the greater or smaller amount of blood-corpuscles which it contains, is of a more deep dark or a bright red colour, and is known as lochia rubra seu cruenta; or it is light red, fleshwater-like, and has got the name of lochia serosa. The first kind belongs generally to the two or three first days; the second kind to the third, fourth, and often even to the fifth day after labour.

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* Virchow's *Archiv.* Bd. xxi., 1861. S. 332.

"I have also observed that from the third day, only a pale-red secretion was discharged, the lochia cruenta consequently completely disappearing.

"The secretion on these days contains for the most part many suspended flocculi, has a peculiar (faden) smell and alkaline reaction.

" *The microscopical constituents are—*

" 1. Blood-corpuscles.

"2. Epithelial scales, elliptical and polygonal, having an appearance of being granulated, or clear and having a nucleus; they compose the flocculi.

" 3. Mucus-corpuscles.

" 4. Mucus-granules, and considerable aggregations of granules.

" 5. Vestiges of decidua and placenta were twice found.

" *Chemical constituents*—

"*a.* Organic—
  Albumen (albuminate of soda).
  Mucin.
  Saponifiable fat.
"*b.* Inorganic—
  Chlorides.
  Phosphate of an alkali.
  Iron.
  Lime-salts.

"The fixed constituents varied from 267 to 86 per 1000.

"III. From the fifth day to the seventh or eighth the secretion is often still partly of a serous nature.

"1. The blood-corpuscles have diminished in quantity, and continue to diminish from day to day; their contour is generally indented, their shape changed, their appearance pale. On the other hand, the secretion has acquired a new microscopical constituent, which quickly increases in quantity from day to day, and at last gains the preponderance. This is

"2. Pus-corpuscles. The secretion maintains a mixed colour, varying from dirty brown, through chocolate-brown, rust-coloured, brick-red, yellow-red, yellow-reddish, to yellow-white or grey colour.

"The smell of the secretion is peculiar and disagreeable; the reaction of the serum generally neutral.

"Besides the enumerated microscopical constituents, there are still found in the secretion in these days—

"3. Epithelial scales; and now the polygonal and elliptical forms are displaced gradually by the occurrence of the round.

"4. Granular cells.

"5. Granules and fat-drops.

"The fixed constituents vary between 108 and 293 per 1000.

"IV. From the eighth or ninth day until the end of the flow, the secretion maintains the same constitution, on the understanding that no farther bleeding supervenes; it has a greenish-yellow, white, or grey appearance, the consistence of cream, and is of neutral or acid reaction.

"*Microscopical constituents*—

"1. Pus-corpuscles, the paramount constituent.

"2. Epithelial scales have greatly diminished in number, and they are generally found only round, larger or younger; later, after the fourteenth day, they are again elliptical or polygonal.

"3. Nucleated cells.

"4. Spindle-shaped tailed corpuscles of young connective tissue, enclosing fatty granules; they are observed on the eighth, ninth, tenth, eleventh, and twelfth days.

"5. Granules and fat-globules.

"6. Crystals of cholesterine.

" *Chemical constituents*—

" *a.* Organic—
  Albumen (albuminate of soda).
  Mucin.
  Saponifiable fat.
  Cholesterine.
  No pyin.
  Volatile acids, inconstant.
" *b.* Inorganic—
  Chlorides.
  Lime-salts.

"The fixed constituents are, from 8th day, 173 per 1000.
„ „ 15th „ 136 „
„ „ 21st „ 144 „

" 7. The volatile acids can only be made out when the reaction of the secretion is strongly acid, so that a strip of damp blue litmus paper hanging free on a cylindrical glass is reddened on the application of a slight heat or without it.

" 8. Products of decomposition, as ammonia and crystals of triple phosphate, which Scherer describes, I have not observed, nor the development of sulphuretted hydrogen

" 9. Clots of fibrin are found in the lochial secretion only when a fresh bleeding occurs in the uterus.

" 10. The peculiar disagreeable smell of the secretion is almost constant from the fourth or fifth day; only it is occasionally absent or weak; it did not always appear to me to be of the same quality; different in the fourth or fifth day to the eighth or ninth from what it generally is in the later days, when it recalls the smell of pus. It remains to be proved whether the smell depends on a volatile acid whose presence cannot always be proved.

"11. Scherer, who supposes that in the lochial secretion there is an epithelial exfoliation of the whole sexual system, and consequently of the uterus, says he has found in the secretion cylindrical and ciliated epithelium; and Kaspar notes the same thing. I have never met with these forms as constituents; their appearance in the lochial secretion is impossible, according to what the physiological part of this work shows regarding the interior surface of the puerperal uterus.

"12. A not constant, but rather accidental microscopical, constituent of the lochial secretion is the trichomonas vaginalis. It has certainly an unmistakable resemblance to cylindrical-ciliated epithelium, for which many have taken it, and contested its existence as a distinct being. Its occurrence in the lochial secretion is not uninteresting, for here it is impossible to confound it with cylindrical-ciliated epithelium.

"13. It has still to be noticed that the blood-corpuscles frequently disappear from the secretion very early, at the sixth day; at other times very late, as first at the twelfth day; that at different periods of the lochial secretion they appear in increased quantity, in consequence of bleeding; lastly, that the mucous viscid stringy constituent is observed sometimes in greater, sometimes in smaller quantity."

CHAPTER VI.

NOTES ON THE HISTORY OF THE MUCOUS MEMBRANE OF THE BODY OF THE UTERUS. WILLIAM AND JOHN HUNTER.

THE fame of William Hunter, although sustained and elevated by his contributions to various branches of medical science, and by the establishment of a great anatomical school, rests chiefly on his labours in obstetrical anatomy. His immortal work, "The Anatomy of the Human Gravid Uterus, exhibited in Figures," is one of the stable foundations of the science and art of midwifery, and cannot fail, in all future ages, to be as valuable and useful as it now is. When, in addition, we consider the scope and tenor of William Hunter's public teaching of midwifery, as handed down to us by his pupils; we shall not hesitate to place his name among the first in the long catalogue of obstetric renown: William Hunter, moreover, was a Scotchman, and a pupil of our illustrious Cullen and Monro. Circumstances, such as these, not to speak of the claims of justice, surely call upon the cultivators of obstetrics to wipe off from his fair reputation any blots with which literary and historical injustice may have tarnished it.

The more this great work on the anatomy of the gravid uterus is studied, the more extraordinary and unparalleled will its accuracy be found to be in every particular. The researches and discoveries of recent times, in regard to the uterine mucous membrane in pregnancy and after labour, will be found, for the most part, to be rendered here, long years ago, as perfectly as they now are or can be.

Almost identical remarks may be made in regard to the volume of text, written by William Hunter, and published after his decease. The wonderful simplicity and accuracy of this book, form the best arguments for its authenticity. In criticising it, however, it will be scarcely fair to attach to its statements a value at all equal to what it would have possessed if published by its author. During his lifetime it had lain long beside him, almost ready for publication, and we have no right to assume that, had his term of life been extended, he would ever have consented to its appearance in the form in which it has become known to the world.

It is from these two works, one published before, the other after, his death, that we draw all our most reliable information regarding this great man's opinions.

It has been truly remarked by M. Velpeau, himself a medical historian and obstetric anatomist of considerable pretensions, that no student examining this part of the human frame could fail to observe the membrana decidua.* But, he adds, such obscure notions as were entertained, scarcely served any other purpose than to impede the researches of observers, and can in no manner be compared to the descriptions of William Hunter.

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* *Traité Complet de l'Art des Accouchments.* Bruxelles, 1835, p. 156.

William Hunter's views, in regard to the decidua, are to be found with absolute authenticity in his plates and his own descriptions of them, and also in a less trustworthy manner in the posthumous volume entitled, *An Anatomical Description of the Human Gravid Uterus and its Contents.*

We can do nothing more, in regard to his plates, than appeal to themselves in corroboration of the most recent and incontrovertible descriptions of the decidua. They are all pictures of real objects, with the exception of figures 7, 8, and 9, of the 34th plate. These last are plans or diagrams intended to illustrate his views in regard to the decidua, and may even now be used with advantage for a like purpose.

In the text explanatory of the plates, he yields to the custom, which still persists, of describing the decidua along with the contents of the uterus, although it be really, as he well knew, an integral part of that organ. For instance, in his explanation of plate fifth, he speaks of "the inner surface of that part of the womb which was in contact with the decidua;" a statement liable to be misinterpreted if not to mislead, unless it be compared with more exact descriptions of the same parts.

His positive assertions in regard to the decidua are, that it is continuous with the substance of the womb (*Descr. of plate* 21); that it is the inside of the womb (*Descr. of plate* 29, *fig.* 1); that it forms the uterine part of the secundines (*Descr. of plate* 33, *fig.* 5); that it forms the uterine part of the placenta (*Descr. of plate* 33, *fig.* 5); that it is not extended across the passage in the neck of the womb (*Descr. of plate* 28, *fig.* 1); that it is continued down into the inner membrane of the cervix (*Descr. of plate* 25); that the Fallopian tubes are not closed by it but open into its cavity (*Descr. of plate* 34, *figs.* 3, 5, 6, 7); that in the early weeks it is a thick membrane of a soft or gelatinous texture (*Descr. of plate* 34, *fig.* 6); that it is abundantly supplied with uterine arteries and veins (*Descr. of plate* 24, *figs.* 3 and 4, also *plate* 27, *fig.* 2); that it has a cribriform or punctated surface (*Descr. of plate* 29, *fig.* 2, also *plate* 34, *fig.* 4); that the decidua reflexa is continuous with the decidua vera (*Descr. of plate* 29, *fig.* 1, also *plate* 32, *fig.* 2, also *plate* 33, *fig.* 1); that the decidua reflexa is permeated by vessels (*Descr. of plate* 27, *fig.* 2); that the reflexa thins as it becomes more distant from the placenta (*Descr. of plate* 28, *fig.* 1), and that it becomes thin from extension, in consequence of the growth of the ovum (*Descr. of plate* 27, *fig.* 2).

In the time of William Hunter, histological pursuits had scarcely been commenced; and we cannot expect that he should have pointed out the microscopic elements of the decidua, the ciliated and cylindrical characters of its original epithelium; the early disappearance of the cilia in pregnancy, and the changes of the shape of the epithelial cells themselves, the characters of the test-tube glands and of their contents. But, apart from these microscopical details, his descriptions may be justly characterised as all true, and as containing all the truth. Numerous authors in our own day, especially E. H. Weber, Sharpey, and Coste, have acquired a harvest of fame for re-discovering and proclaiming what we have shown that William Hunter demonstrated and described in 1775, and that not casually and carelessly, as if he had stumbled unexpectedly on truth, but carefully and with reiteration.

No sooner do we leave the guidance of William Hunter, than we fall into a long-continued tissue of errors, of blunders, and of misrepresentations.* I am not aware of any instance of such retrogression from truth to untruth, from clearness and simplicity to doubts and confusion, in the history of any science continuously pursued by men of zeal and ability; and, in the present instance, the heart is touched with a feeling akin to pain, when we reflect that the leaders in this unfortunate direction were the nearest relatives of William Hunter—viz. John, his brother and his pupil, and Matthew Baillie, his nephew; for both of whom, it is fortunate that, in other quarters, there is an ample reserve of well-won fame.

After the death of William Hunter, on Sunday the 30th March 1783, the manuscript of a work, which he had, in the preface to his volume of plates, announced as nearly ready for publication, came into the hands of his nephew, the justly eminent Matthew Baillie. With a becoming modesty, which must now shield him from all severity of blame, he delayed the publication of the book, because, to use his own words, he "had studied anatomy for so short a time, and indeed was so young, as not to be capable of judging whether the manuscript was in a state fit for publication or not." But on actually producing it, he states, in a preface, that "what appeared to him to be wanting, he has attempted with much diffidence to add, but this amounts only to a few pages;" a resolution as unfortunate for obstetric anatomy, as it was ill-advised in him. This circumstance introduces an element of justifiable diffidence in the authenticity of the words of this volume, entitled *An Anatomical Description of the Human Gravid Uterus and its Contents*, and published in 1794.

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* See some remarks by the author in the first Chapter of this Part.

The following are the chief and most authentic passages in this book on the subject of the decidua. I quote from the first edition:—

"This membrane is an efflorescence or production of the inner membrane of the uterus, and is analogous to the uterine fungi of quadrupeds. It receives no vessels demonstrable by the finest injections from those of the navel-string, yet it is full of both large and small arteries and veins. These are all branches of the uterine vessels, and are readily filled by injecting the arteries and veins of the uterus; and they all break through on separating the placenta from the uterus, leaving corresponding orifices on the two parted surfaces.

"This decidua, or uterine portion of the placenta, is not a simple thin membrane expanded over the surface of the part; it produces a thousand irregular processes, which pervade the substance of the placenta. . . ." (P. 42.)

"It is the outer membrane of the secundines, and yet it may be said to be the internal membrane of the uterus." (P. 54.)

"Though the decidua be allowed to be the outer membrane of the secundines, yet as it is really the internal lamella of the uterus, we may still retain the old language and say, that the outer membrane of the ovum (that is, of the contents of the uterus) is chorion,* and that the chorion is in contact with and adheres to the uterus." (P. 57.)

In commenting on these latter passages from Dr. William Hunter's posthumous volume, Dr. Rigby, who has edited an excellent edition of it (to which our paginal indications elsewhere refer), truly remarks that such "expressions scarcely justify us in attributing to them the meaning of its being merely an effusion of coagulable lymph." But the additional evidence now adduced, from the authentic descriptions attached to the volume of plates, enables us to go much further, and assert, that it is trampling on justice to ascribe to William Hunter any views in regard to the decidua which are not quite correct.

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* It is necessary to explain that in Hunter's time the decidua was often called chorion, or spongy chorion. It is evident that in this sentence the author indulges in a play upon the word "chorion," used by himself to imply the outer membrane of the ovum. In this sentence only, and for the nonce, and in accordance with a custom in his time, he uses it to imply the outermost layer of the contents of the gravid uterus or decidua.

Passing on, in this history, from the observations of William Hunter, we fall among errors of description founded on bad observation, and such as could not have been conceived and propagated without unsoundness of judgment.

In the year 1786, the great John Hunter published a volume, entitled *Observations on Certain Parts of the Animal Economy.* This book contains a paper "On the Structure of the Placenta." "This paper," says John Hunter, "was read at the Royal Society; but as the facts had, before that time, been given to the public, it was not published in the *Philosophical Transactions.*" But this bare statement gives an insufficient view of the circumstances of this paper.

William Hunter had already acquired fame in connection with the discovery of what may be called the Hunterian anatomy of the placenta. He had taken to himself the merit of the discovery, by omitting the name of any other anatomist in giving an account of it in the description of his plates of the gravid uterus. John Hunter felt aggrieved at his brother's conduct in this matter, and an unfortunate estrangement between the brothers was the consequence. The quarrel first became public in 1780, in consequence of John's presenting to the Royal Society his paper on the placenta. The Society refused to publish John Hunter's paper, or to interfere in the matter. William addressed the Society a letter vindicating his own claims. John sent to the Society an answer, in which he appears at least to be generous, for he professes that he will be satisfied with one-third of the merit, leaving to his elder brother and to Dr. M'Kenzie the other shares. His conduct in this particular may be viewed in two different and opposite lights. I shall only remark, that reflection on the famous judgment of Solomon will not tend to confirm the long-delayed claims of John.

This episode is here introduced, partly because of the important bearing upon it of the remarks now to be made. It has long been my humble opinion, that while the fame of John Hunter has not exceeded his solid deserts, it has, partly from the greater range of subjects on which it is founded, and partly from the coincidence of the surnames and the literary neglect of the christian names, injuriously overshadowed the colossal merits of William. In the point as to which the brothers unfortunately disagreed, it is surely not a matter of small importance, the dispute being as to originality, to note the simplicity and accuracy of William, while, on many important parts of the same subject, John is visionary and inaccurate.

In 1780, then, John Hunter inaugurated the errors in regard to the decidua which have been finally overthrown only in our own time, and which still maintain a lingering existence in obstetric literature. The paper read to the Royal Society in 1780 was published in 1786, in his work entitled *Observations on Certain Parts of the Animal Economy.*

"At the time," says John Hunter, "or very probably before the female seed enters the uterus, coagulable lymph, from the blood of the mother, is thrown out everywhere on its inner surface, either from the stimulus of impregnation taking place in the ovarium, or in consequence of the seed being expelled from it. When the seed has entered the uterus, it attaches itself to that lymph, by which it becomes covered and immediately surrounded. This coagulable lymph forms a soft pulpy membrane, the decidua, which is, I believe, peculiar to the human species, and to monkeys, having never found it in any other animal. That part which covers the seed or fœtus—where it is not immediately attached to the uterus, and likewise forms a membrane—was discovered by Dr. Hunter, and is by him called decidua reflexa. The whole of this coagulable lymph continues to be a living part for the time; the vessels of the uterus ramify upon it; and where the vessels of the fœtus form the placenta, there the vessels of the uterus, after passing through the decidua, open into the cellular substance of the placenta, as before described." In a note, he says, that "this is exactly similar to another operation in the animal economy. If an extraneous living part is introduced into any cavity, it will be immediately inclosed with coagulable lymph. Thus we find worms enclosed, hydatids detached and afterwards enclosed." (P. 133 of 1st edition.)

To this error, in describing the decidua to be an exudation of coagulable lymph, he subsequently added others, in a note in the edition of the *Animal Economy* published in 1792, namely, in apparently describing the placenta, as entirely a fœtal part, in doubting or denying that the decidua reflexa is an uterine part, and in describing a doubling of the decidua. He says—

"The placenta is certainly a fœtal part, and is formed on the inside of the spongy chorion or decidua. How far the decidua reflexa is a uterine part I do not yet know; if it is, then the ovum must be placed in a doubling of the coagulum, which forms the decidua; but if the ovum is attached to the inside of the decidua, then the decidua reflexa is belonging to the fœtus."

Further evidence is not required in regard to John Hunter's views. But it may be well to quote his description of the uterus of a woman dying after a supposed recent impregnation. The specimen was furnished to him by Mr. Thoms Ogle; and John Hunter's account of this membrane is in notes by him, appended by Mr. Ogle to a paper read on August 5, 1794, before the *Society for the improvement of Medical and Chirurgical Knowledge,* and published in 1800 in the second volume of that Society's Transactions. In Palmer's edition of John Hunter's works, published in 1837, this paper is further annotated by Richard Owen, who gives in his adhesion at that date, to the erroneous views of John Hunter. It will be observed, that here

John Hunter speaks of coagulated blood, not as previously, of coagulable lymph; and he makes the new error of describing the decidua as closing the os uteri.

"The uterus," he says, "was unusually soft in texture, and terminated on the internal surface in a pulpy substance.

"The blood-vessels of the uterus passed into and ramified upon this pulpy substance, which was continued across at the cervix uteri, so as to make the cavity of the uterus a circumscribed bag; and at this part the pulpy substance was so thin as to resemble the retina.

"This cavity had a smooth but irregular internal surface, and the pulpy substance upon which it was formed was evidently blood coagulated and varied in its thickness in different parts. Upon a longitudinal section of the uterus, the posterior part of the coagulum, which was the thickest, was nearly half an inch; where it terminated towards the cervix it was pendulous and unattached. There were also several loose processes, all turned towards the cervix, one of them very thin, as broad as a silver penny, and only attached by one edge to the fundus near the opening of the right Fallopian tube.

"On slitting open the Fallopian tubes, the coagulum was found to pass some way into them, and to extend more than half-an-inch on the left side, which had the corpus luteum. The coagulum was thickest at the orifice of the tube, and then adhered to the inner surface for the eighth part of an inch, beyond which it became smaller, and terminated in a point. In the left tube the coagulum was in two places coiled or folded upon itself, as if thrown back by the action of the tube. The portions of the coagulum at the orifices of the tubes were hollow."

Throwing upon these descriptions the light of modern science, we can now easily discern several great errors. By some commentators they have been attributed to Matthew Baillie;* but he is really safe from this imputation. His edition of William Hunter's book was published in 1794; that is, long after his famous uncle John had given them currency and the stamp of his recognisal. But Baillie is himself not without blame. At p. 77 of his edition we find the following footnote:—"Here ends the manuscript of Dr. Hunter, except that what is afterwards said about the navel-string is also of his writing. The editor has taken the liberty of transposing this from the place where it was, to another which appeared to him more proper for it." If, then, we look at the part of the book following this note, we shall find the errors of John Hunter appearing in the text; a circumstance which affords an easy explanation of how William's reputation has come to be injuriously burdened with them. This last part of the book, and a few footnotes, are editorial. Matthew Baillie is responsible for what they contain; and it scarcely requires a critical eye to discern, that the whole tenor of them is less simple and accurate than the rest of the work.

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* See Rigby's edition of the *Anatomical Description of the Human Gravid Uterus.* Footnote, p. 48.

The circumstance that Matthew Baillie has never been held an authority on this subject, and a desire to avoid tedious lengthiness, might afford sufficient excuse for neglecting to adduce the evidence of passages. But I have resolved to be complete, at the risk of being tiresome. In a spurious part of the text of William Hunter's book we find the following:—

"The decidua resembles a good deal in its appearance, as well as in its mode of formation, the lamina of coagulable lymph, which is formed by inflamed surfaces. Both membranes are of a yellowish-white colour; both are tender, pulpy, and vascular. The lamina of coagulable lymph is formed by an inflamed membrane; the uterus, before the decidua is formed, becomes much more vascular, so as to change into a state somewhat analagous to inflammation. The points of comparison, however, between these two membranes reach no farther. The lamina of coagulable lymph is gradually changed into the membrane of adhesion, which resembles exactly the common cellular membrane of the body; but the decidua continues throughout a peculiar membrane.

"How the decidua envelopes the ovum has never yet been observed, and therefore can only be a subject of conjecture. The most probable supposition is, that the ovum passes from the ovarium into the cavity of the uterus, while the coagulable lymph is pouring out by the arteries of the uterus, which is afterwards changed into decidua." (P. 82.)

In a footnote, again (p. 79), we find Baillie indorsing John Hunter's observations :—

"In a case," says he "of very early conception, probably not more than two weeks, which was examined by Mr. Hunter, the decidua was found, upon opening the uterus, to be as fine at the beginning of the cervix as the retina, but without any hole in it there. This, perhaps, always takes place in a very early conception, where the ovum remains undisturbed in the uterus; but when it passes off in a miscarriage, the decidua at the cervix is perforated. In more advanced pregnancy, that part of the decidua which lines the inner surface of the uterus, and which will, in the progress of the description, be distinguished by the name of the decidua vera, seems to lose itself at the beginning of the cervix, and has evidently there an opening. The decidua, which covers the external surface of the chorion, becomes gradually thinner as pregnancy advances, but has no opening at the cervix uteri, or anywhere else."

So much for Dr. Baillie. Were I now to attempt to trace the history of John Hunter's teaching on this point, down to our own day, almost every book on human physiology and on midwifery would require to be cited. And it is scarcely to be wondered at that such a great name should have made so profound an impression on medical literature. In most of the works referred to, the erroneous views of John are ascribed indiscriminately to the two brothers, or especially to William; an act of injustice to his memory which we may be permitted to hope will now be, in some degree at least, redressed.

Various authors have introduced into the history of this part new errors, to which the present satisfactory condition of the subject renders it unnecessary to attend particularly.

To have established correct views in regard to the decidua, to have re-established all the teachings of the great William Hunter, is one of the principal achievements of modern physiological science. To this end the researches of numerous anatomists have contributed, and the results attained are partly due to the assistance of the microscope. It is, indeed, scarcely conceivable that the views of William Hunter could have been so soon reintroduced, had they not been supported by the novel and irrefragable evidence furnished by histological research.

Physiology, then, now teaches that the decidua vera is an uterine membrane, is the mucous membrane of the uterus; that in the early months its surface is cribriform, from the presence of numerous openings of ducts, that none of the natural orifices of the uterus is closed by it, unless, indeed, the ovum is inserted over one or other of them.* The decidua reflexa is also now known to be a process or production of the parietal decidua after the ovum has entered the uterus, and its history accords entirely with the indistinct, though correct outline, found in William Hunter's writings.

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* The insertion over one or other Fallopian tube is sufficiently common, though opportunity is not often afforded of ascertaining it. The insertion over the os uteri forms the condition of placenta prævia. In Wm. Hunter's work, almost the earliest possible case of this is depicted in an abortion about the fourth week.

The true anatomy of the decidua, now satisfactorily established, is only beginning to produce its rich crop of fruits for the science and art of midwifery.

Before concluding, I have a few remarks to make in regard to the condition of the interior of the uterus in dissections of the gravid organ, and after early abortion or delivery at the full time. These subjects have been sources of much error in obstetrical and physiological works, and I allude to them now in order to point out the important circumstance, that William Hunter long ago correctly described them.

The descriptions, by Albinus and Noortwyk, of dissections of gravid uteri, are very vague, at least so far as regards its internal surface. But William Hunter is quite distinct. In describing the dissection of a gravid uterus from a subject at the seventh month, he specially notices the behaviour of the decidua.

"In this angle," he says, "between the womb and secundines, the artist endeavoured to express what was very apparent in this object, viz., the continuity of the substance of the womb and of the secundines ; in parting which, the tender connecting medium, the decidua, separated into two layers, one of which clung to the womb, and the other to the chorion."—*Descr. of plate* 21.

Again, in the posthumous volume we find the following remarks :—

"I afterwards had the most favourable occasion that could be desired for examining the (muscular) fibres upon the inside of the uterus. It was the uterus of a woman who died at the end of the ninth month without being in labour, and without having any flooding or discharge of waters. When I had examined and taken out all the contents, I attended particularly to the internal surface of the uterus. I found it everywhere covered with a thin stratum of the decidua, through which the muscular fibres appeared, but with some degree of obscurity. Upon rubbing off this tender membrane with a cloth, it gave me pleasure to see how exactly the above description agreed with the appearances."—*Anatomical Description*, etc., p. 28.

" In another passage (p. 56), he makes the general remark, that—In separating the membranes from the uterus, we observe that the adhesion of the decidua to the chorion, and likewise its adhesion to the muscular fibres of the uterus, is rather stronger than the adhesion between its external and internal stratum, which we may presume is the reason that in labour it so commonly leaves a stratum upon the inside of the uterus."

There is no special passage to quote from William Hunter's works in regard to what occurs in abortion. His 34th plate is, however, so accurate, that we have no reason to presume that his views were only in this point erroneous. In early abortions it is well known that the decidua reflexa is discharged with the ovum as well as the internal or superficial layer of the decidua vera, a layer of this latter being left on the surface of the uterus. In membranous dysmenorrhœa, when a characteristic membrane is discharged, it is anatomically the same part of the parietal uterine mucous membrane that comes away. The analogy between this expelled substance in dysmenorrhœa and early abortion, and the condition of the remaining uterine surface, I have pointed out in another place.*

In regard to the internal surface of the uterus after delivery, William Hunter's observations are far from being so full and distinct as on other subjects to which we have referred. In one place, indeed, he seems (*Anat. Descr. p.* 27) to confirm the later, and, I believe, erroneous view of Cruveilhier and other morbid anatomists who describe the muscular fibres of the uterus as lying bare. But when we reflect that here he is speaking probably of a morbid specimen, and one which had undergone various processes of preparation, we shall not be inclined to hold him as believing that what only appeared to be, in this case, is the healthy and ordinary condition, especially when everywhere else his descriptions indicate that he held more correct views. The questionable passage is as follows:—

"In a woman who died seven days after delivery I gave up the uterus to this pursuit, and examined the fibres very carefully. I stretched it gradually in warm water, then inverted it, to have a full view of its inner surface. The remains of the decidua had been melted down and passed off with the lochia, so that the fasciculated stratum of muscular fibres appeared to be bare, and to make the internal surface of the uterus."

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* See page 214.

Passing from this doubtful extract, we come to places where he is more exact. Describing a plate showing the muscular fibres, he says :—

"The part was steeped in water some days, whereby the decidua was made tender, and then brushed off." (*Description of plate* xiv.)

Again, in describing the womb in a woman who died immediately after delivery, he points out—

"Part of the inner lamella of the womb, raised by dissection and turned to one side, to show the fasciculated fibres of the womb." (*Description of plate* xv. fig. 1.)*

A more distinct passage still occurs in the *Anatomical Description*, etc. (p. 55), and one which can scarcely leave a doubt that on this subject also, William Hunter promulgated truths which were destined long to lie concealed or disregarded among the errors of his successors. Speaking of the decidua he says :—

"This membrane is an efflorescence of the internal coat of the uterus itself; and is therefore shed as often as a woman bears a child, or suffers a miscarriage. It is of considerable thickness, and one stratum of it is always left upon the uterus after delivery, most of which dissolves and comes away with the lochia."

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* This mode of showing the muscular fibres has been imitated by Sir Charles Bell and by Bourgery, but apparently in ignorance of the nature of the layer they thus removed.—See *London Medico-Chirurgical Transactions*, vol. iv. p. 341, and Bourgery's description of figures 3 and 4, plate 75, vol. v. of his *Great Atlas of Anatomy*.

CHAPTER VII.

ON THE LENGTH OF THE CERVIX UTERI IN ADVANCED PREGNANCY.

THE condition of the cervix uteri at different periods of pregnancy is a subject not only of great physiological interest, but has also many practical bearings of the highest importance. My remarks will be confined to those conditions of the neck of the womb which have long held a prominent place in the descriptions of pregnancy. To Stoltz is generally awarded the credit of introducing the new views which I am about to defend; yet I shall hereafter show that he was long anticipated by well-known anatomical and obstetrical authors. Passing these over in the meantime, their accurate observations having not only been forgotten but supplanted by the still prevailing errors, we come to the thesis sustained by Joseph-Alexis Stoltz, to obtain the degree of doctor of medicine from the Faculty of Medicine of Strasbourg. This thesis, entitled *Considérations sur quelques points relatifs a l'Art des Accouchemens*, was published in 1826, and contains, as its first part, an article "Sur les différens états du col de l'uterus, mais principalement sur les changemens que la gestation et l'accouchement [illegible] gives in his thesis only the results of examinations as made at the [illegible] of Accouchement of the Strasburg Faculty. My present object is to elucidate the views of Stoltz with some modifications, and especially so to connect them with actual anatomical observations as to make it

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* Journal des Accouchemens, tome [illegible] p. 178.

imperative on obstetricians to accept them implicitly, or disprove them by other observations as carefully made.

In discussing this subject, I intentionally omit the latter days of the ninth month of pregnancy. During these days, which are included in the full term of utero-gestation, silent and painless labour may be said to be really going on;—I mean, that contractions of the uterus, usually without pain, are effecting the complete obliteration from above downwards of the cervical canal. Although generally painless, these contractions frequently attract attention, and not rarely cause a degree of suffering. When they do cause pain, they give rise to many of those occasional cases where women describe themselves as having been in labour for many days. It is, therefore, not only convenient, but perhaps, also, physiologically correct, to exclude all this rapid change in the lower part of the uterus produced by contractions from among those conditions of pregnancy which we propose now to discuss.

This occasional source of pain and protraction in the first stage of labour at or near the full time, is described by Weitbrecht as the cause of like evils in cases of abortion and miscarriage, and as his words throw light on his views as to the subject now under discussion, I here quote them. "Neque minus ratio patet (aliis causis tamen neutiquam posthabitis), quare mulieres, quæ primis vel mediis mensibus abortum patiuntur, doloribus multo vehementioribus [illegible] maximum et [illegible] mulieribus communem."*

Although, as I have already said, Stoltz everywhere gets the credit of introducing the new doctrines, I shall first, in an imperfect historical sketch, show that while plenty of honour is to be awarded to Stoltz, he was anticipated by other observers. In the *Icones Uteri Humani* of Roederer, the celebrated Göttingen professor, a beautiful picture is found of a cervix taken from a woman six months advanced in pregnancy. In it the whole cervix is seen to retain its length of about an inch, and to be considerably hypertrophied. It certainly does not, in this case, as yet at least, contribute by its development to the formation of the lower half of the cavity of the uterus. Roederer, however, held the erroneous doctrine, that after the fifth month the cervix is gradually expanded from above downwards, and contributes to the formation of the cavity of the pregnant uterus. In his *Elements of Midwifery*, he describes as follows :—" Ita ad ulteriorem expansionem cervix præparatur, quæ præcipue versus sextum mensem notari meretur, quo quidem tempore eousque diduci incipit, ut cum communi uteri cavitate tubi pars orificio superior confundatur, ex qua mutatione etiam brevius fit uteri orificium, labia sensim ad aperturam exteriorem diducuntur, figura conica vel cylindracea deletur."* The fact that his doctrine is not

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* *Novi Commentarii Acad. Sc. Imp. Petropol.*, tomus i. p. 349. 1750.

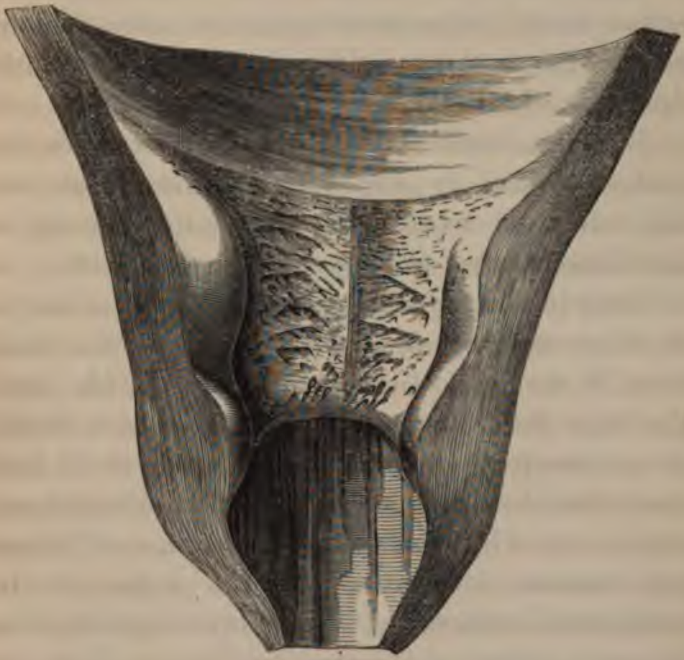

Fig. 20.

Fig. 20. Copied from Fig. 6. of Tab. VI.

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* *Elementa Artis Obstetriciæ.* Gottingæ, 1759. P. 23.

[illegible] regarded [illegible]

[illegible] finger. [illegible] finds [illegible] Illustrations show the reverse, so far as I know. [illegible] work has two illustrations which illustrate this point, and both are antagonistic to his quoted assertion. (One of these illustrations (Tab. VI. Fig. 6.) has already been given. The other (Tab. VII. Fig. 1.) is still more destructive of his proposition. It is a picture of the internal surface of the cervix uteri of a woman who had been twelve hours in labour at or near the natural end of pregnancy. It shows just such

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* *Icones*, p. 30.

mechanical changes in the cervix as might be expected to take place under the circumstances, and, so far as it goes, is adverse to the doctrine espoused by Rœderer, and supports that which I am now demonstrating.*

It would be very interesting to cross-question Rœderer regarding his assertion that dissections confirm the view which he thus states,—"Mirum itaque non est, primis mensibus explorantem digitum nihil mutationis ad cervicem notare, et quinto demum a conceptione mense tumentem sensim pone os uteri cervicem offendere, per lentos inde gradus cum integra uteri ellipsi confusam." In this passage it is evident that, as with Stoltz, the examining finger holds the chief place as the source of information. His own dissections, as we have seen, do not confirm the suggestions of his examining finger, and it will be very interesting to analyse the observations of other anatomists [Verheyen, de Graaf, Weitbrecht] whom we know he consulted on this point. Remarkable to relate, they all oppose his view; and one of them, Weitbrecht, as we shall see, does so more authoritatively than any author whose works I know.

In the anatomical work of Verheyen,† Professor of

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* The conditions of the cervix during labour and shortly after delivery, especially its elongation from above downwards, do not naturally come to be considered in this paper. But they call for the attention of obstetricians with a view to the completion of the history of this interesting part. For some observations on this elongation, which I pointed out some years ago, see the work of Dr. Ed. Martin. *Die Neigungen und Beugungen der Gebärmutter nach vorn und hinten* Berlin, 1866. S. 44.

† *Corporis Humani Anatomia*, p. 126.

[illegible] which was [illegible] in [illegible], we find the following [illegible] —"In gravidis [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible]." Again, speaking of the [illegible] of the [illegible] of the uterus :—"[illegible] [illegible] [illegible] [illegible] in non gravidis, [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible]. In gravidis [illegible] [illegible] usque ad partum [illegible]." Again, further on :—"Cervicis uteri [illegible] est, et [illegible] [illegible] maxime [illegible] [illegible] quae [illegible] [illegible] [illegible] [illegible] [illegible] in [illegible] [illegible] menses [illegible] [illegible] [illegible] [illegible] [illegible]. In gravidis autem [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] propriam [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] copia [illegible]." And then he describes the inferior part of the cervix as it projects into the vagina. A perusal of these passages can leave no doubt that Verheyen's views of the anatomy of the cervix uteri did not accord with those of Roederer, who speaks as if they did so. They coincide with the most modern descriptions.

The famous de Graaf does not address himself to the special point before us in a special way, but what he does say agrees with the statement of Verheyen. Weitbrecht,* indeed, suggests that de Graaf meant, in his statement to be quoted, to refer to the uterus only in the first half of pregnancy. I can see no reason for this view; but Weitbrecht had perhaps private or other means of knowing de Graaf's opinions. At the same time, just principles of criticism lead us, in the circumstances, to adopt de Graaf's statement without the comment of Weitbrecht. In the eighth chapter of his work on the *Female Organs of Generation*, he says:—"Collum enim (quod omnino notandum est) dilatationem uteri non insequitur, at pristinum fere statum usque retinet; idque non in hominibus solum, verum in vaccis, ovibus, aliisque animantibus indies evenire conspicimus."*

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* "Primo quidem abunde confirmatur assertum Graafii qui stabilivit 'Collum non insequi dilatationem uteri gravidi, sed pristinum fere

Unlike de Graaf, Weitbrecht (1750) has entered fully and particularly on the condition of the cervix uteri in pregnancy in his memoir, *De Utero Muliebri*, communicated to the Imperial Academy of Sciences of St. Petersburgh. His descriptions are full and careful, and leave, in respect to the subject now under discussion, nothing to be desired. And it must be distinctly stated, to the great honour of Weitbrecht, that it is to him chiefly that the credit of stating and illustrating by a dissection the true doctrine of the development of the cervix in pregnancy is due. Henceforth it will be a grave error to consider the true account as modern and as a discovery of Stoltz,

statum retinere,' id quod de mediis gestationis mensibus intellectum vult." *De Utero Muliebri Observationes Anatomicæ.* Auctore Josia Weitbrecht. *Novi Comment. Acad. Sc. Imp. Petropolitanæ.* Tom. i. p. 348.

* *Opera Omnia Regneri de Graaf.* Lugd. Batav. 1677, p. 233.

re-discovered or confirmed by more recent authors. Rœderer and his followers down to this day have to bear the responsibility not merely of taking the feelings acquired by the examining finger as anatomical facts, but of doing this in spite of anatomical facts and descriptions of whose existence they were aware. No doubt Rœderer asserts that dissections prove or confirm his view, but he does not state where they are recorded. I believe they do not exist, and that he and his followers commit, though with less excuse, the scientific fault of Galen, who describes the human uterus which he never saw, and of course encumbers his pages with gross errors. Obstetrical students should peruse the whole essay of Weitbrecht. I can only quote short passages in proof of what I have just said. The chief subject of it is the dissection of the uterus of a woman seven months pregnant.

"Cervix, sive collum uteri non exigua hujus organi portio est. Sed in statu prægnante non in eadem temporis proportione mutationibus et extensioni obnoxiam esse ac fundum, observationes nostræ luculenter docuerunt. In virgine et vetulis dimidiam propemodum longitudinem totius uteri, quæ tum notum est, vix duos pollices æquat, compleverat. In prægnante perparum ab hac forma et quantitate recesserat, nisi quod ante dissectionem considerata habitum paullo turgidiorem præ se ferret et duritie fundum superaret. Contra uteri cavi longitudo erat ultra octo pollices . . . .
Hæc tota specus ex tam spaciosa amplitudine coarcta-

batur inferius in foraminulum adeo exiguum, ut vix pisum admitteret, ceu osculum urethræ internum ex contractis vesicæ urinariæ tunicis generatur. Hoc foraminulum vocare placet osculum cervicis internum, ut distinguatur ab altero vulgo cognito, os uteri dicto, transversa rima in vaginam hiante, quod osculum cervicis externum appellabimus. . . . .

"Postquam cervicem, continuata uteri sectione longitudinali, aperui: tota distantia ab uno osculo ad alterum pollicem circiter æquaverat. . . .

"Quæ hactenus de uteri cervice annotavimus, ad multas veritates viam nobis pandunt. Primo quidem abunde confirmatur assertum Graafii qui stabilivit 'collum non insequi dilatationem uteri gravidi sed pristinum fere statum retinere' id quod de mediis gestationis mensibus intellectum vult. Cum natura rei igitur plane non congruit idea illorum, qui uteri prægnantis cervicem sibi fingunt ceu unicum osculum, annulo quasi membraneo occlusum, qui paullatim mollior fiat et amplior, donec ita hiet, ut fœtum transmittere possit, qualem, *e.g.*, Deventer pingit. Hoc enim non nisi de ultimis diebus graviditatis, quando partus appropinquat et imminet, intelligi debit; tum enim orificium paullatim distenditur et annuli simplicis formam nanciscitur, per quem vix unum alterumve digitum trajicere liceret. Totalis autem dilatatio tum demum, uti obstetricando experimur, locum habet, quando jam parturiens aliquos dolorum prodromos persentiscere incipit, aquæ rumpuntur, et caput fœtus ad cervicis orificium adigitur."

To this account of the behaviour of the cervix uteri in pregnancy there is added an excellent minute engraving of its condition in the patient whose dissection after seven months of pregnancy gave rise to the memoir. It resembles greatly the picture of the same part already referred to as occurring in the *Icones* of Rœderer, and also the following woodcut of

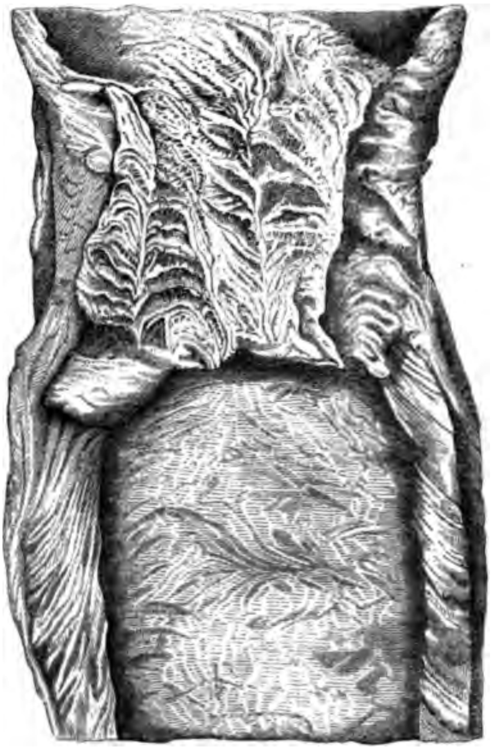

Fig. 21.

Fig. 21. A copy from Fig. II. Tab. XII. of Weitbrecht, *ibid.*

the cervix of a woman who died after eight months of pregnancy (Fig. 28).

Levret's opinions regarding the cervix seem to hold a position midway between those of Weitbrecht and the erroneous notions of modern authors. His words are as follows:—"Le col de la matrice deviênt successivement, plus gros, plus long, et plus mol pendant les sept premiers mois de la grossesse, mais passé ce tems, il commence à se raccourcir, à force de s'évaser, et vers la fin il s'efface peu à peu. . . . c'est vers le milieu de la grossesse, que le corps de la matrice commence à s'étendre manifestement, et ce n'est que vers la fin que le col prête à l'extension."*

It is necessary now to define what limits we assign to the cervix, whose length we propose to measure. Its superior boundary is made by many to be, in the unimpregnated uterus, the most contracted point separating what are called the cavities of the body and cervix. This purely artificial limit may, in some cases, tally with the natural one we propose, along with the best writers, to adopt; but it is impossible in most unimpregnated uteri to fix it, and in the pregnant uterus it would probably be found to indicate a quite different anatomical point from what it did in the unimpregnated. For the natural limit is the only one observed in pregnancy; the parts above it, and they alone, distending as the ovum grows. It is marked by the anatomical differences of the cervix and body of the womb, and is indicated by the union of the rugous mucous surface of the cervix and the smooth surface of the body. The superior boundary is the margin of the os externum, or the lowest line of the projecting vaginal portion. In advanced pregnancy, when, as may happen, the projecting vaginal portion is obliterated, the inferior limit of the cervix is often indicated by the distal margin of the Nabothian follicles. The length of the cervix, it will be observed, is here made considerable, as it affords the only means whereby a distinction of cervix from body can be at all effected,* in the interior of the uterus, for in its fibrous structures there is no indication of difference between these different parts of the organ.

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* *L'Art des Accouch.* etc. p. 64, 1761.

It is not consistent with the scope of my observations to enter at any length into the history of the opinions regarding the length of the neck of the womb in pregnancy that have heretofore been entertained in this and other countries. The doctrine taught in recent times by Desormeaux, Gooch, and Montgomery, still retains its place in the most modern midwifery text-books, including those of Ramsbotham, Churchill, and Tyler Smith. Ever since I began to teach midwifery, I have pointed out the erroneous character of these views, showing that they contained some valuable truths if the sensations acquired by the examining finger were the only criterion, but that, when subjected to a rigid scrutiny they were found to be entirely erroneous. In this respect they resembled the popular notions of the sun's course in the heavens which, although true to the eye, are repudiated by all who wish to be consistent with facts and with nature.

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* The following microscopical observation by Robin is too interesting as a confirmation of the views of this paper to be here omitted. I put it in a foot-note not because it is unimportant but because it is out of keeping with the kind of demonstration used in the text. In his work on the "Uterine Mucous Membrane," published in the *Memoirs of the Imperial Academy of Medicine of France*, he describes the epithelium of the cavity of the cervix as differing from that of the body of the uterus, in retaining its prismatic state during the whole continuance of pregnancy, although, for the most part, it loses its vibratile cilia.

The following extract from the second edition of Dr. Montgomery's work on the *Signs and Symptoms of Pregnancy*, published in 1856 (p. 180), describes clearly the opinion generally received in this country. "It is usual," he says "to state the abbreviation of the cervix by exact proportional parts; and thus, it is said, that during the sixth month it loses one quarter; that in the seventh, it is only one-half its original length; that in the eighth, only one quarter remains, which, in the ninth month, is reduced to an eighth, which is obliterated before the end of that month. Now, all this may be true in very many cases, and I believe it is so, but we can derive from it little or no practical benefit. Such precision is only available with a uterus in a preparation, or on a dissecting-table, but not in the examination of a living woman, etc." To this clear statement of the gradual diminution of length as an anatomical fact, we shall add a few words from the *Manual of Obstetrics* of Dr. Tyler Smith, published in 1858, to show the opinion generally entertained as to the results produced by this shortening. He says (p. 104), "About the fifth which the uterus is found to have been half-way to the umbilicus. It is now that the cervix itself begins to shorten its cavity being taken up into the general uterine cavity by a process of development commencing at the junction of the cervix with the body of the organ, and terminating at the os itself."[*]

Writing on the internal surface of the uterus after delivery in [illegible], I said—"The cervix of the uterus is now shown to differ so materially in its anatomy and physiology from its body, that it may with propriety be described as almost a separate organ. The researches of M. Stoltz have been confirmed by numerous authors, and seem to show that the cervix itself does not become developed so as to form part of the cavity of the organ in the way that it was formerly believed to be. More recent researches by anatomists and obstetricians, too numerous to be mentioned, have shown the great difference, anatomically and structurally, as well as functionally, of the mucous membrane of these two parts."[†]

The opinion of M. Stoltz which Cazeaux corroborates, is that the cervix undergoes during pregnancy, at least in primiparæ, a decided though slight diminution of length; and that this diminution is produced, not by the enlargement and distension of the cervical cavity from above downwards, but by the approximation to one another of its two extremities, the cavity becoming more markedly fusiform and filled with its ordinary mucous secretion. To the refinements of this description by Stoltz and Cazeaux I cannot assent. For, first of all, a slight diminution, even if admitted to exist, can be of no importance, seeing that it cannot in any way be appreciated or measured during life; and it is to be remembered that the observations relied on by these authors were all made on living women. Second, because there is no standard length of the true cervix yet demonstrated, to afford us a just means of comparison in questions of slight changes.*

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[*] This theory will be found graphically illustrated in many obstetric works, affording a fine example of the injuriousness, not to speak of the uselessness, of many cuts, which enlarge the bulk and increase the expensiveness of many modern productions. For examples I may refer to the midwifery text-books of Hohl and Braun. The figures themselves, if studied, are found to be so absurd, as to form a refutation of the doctrine they are intended to illustrate. I must add, that the figures of Cazeaux, illustrating the new views, are not well designed.

[†] *British and Foreign Medico-Chirurgical Review* for October, p. 506.

My own statement, that the length of the cavity of the cervix uteri undergoes little or no change during pregnancy, is founded partly on the results of vaginal examinations, and chiefly on the examination of the uteri of women that have died at different stages of utero-gestation. In some of the figures here given there will be seen an apparent elongation rather than shortening of the cervix.

In regard to the evidence of vaginal examinations, I can merely make the statement, that when carefully made, they support my proposition. Only it is necessary to take precautions against receiving erroneous impressions. These are, firstly, not to be misled by the frequent apparent obliteration of the vaginal portion of the cervix, that being a cause only of fictitious, not real shortening; secondly, not to mistake contracted dimensions produced by the finger's pressure on a softened cervix for real shortening: and, thirdly, to place no confidence in the impressions conveyed by the finger pushing before it the vaginal reflection, and feeling the external surface of the part.* In addition, I would recommend the investigator to measure the length of the cavity by gently intruding his finger through the external os uteri, as can generally be done in advanced pregnancy, in multiparæ, and frequently in primiparæ also. And I would especially insist on the value of examinations made immediately before labour, when the cervix is extremely softened and largely dilated, and its long lips can be felt surrounding the internal os uteri.

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* For some attempts at measurements during life see Schroeder, who refers to Spiegelberg. *Schwangerschaft Geburt und Wochenbett*, S. 12, Bonn, 1867.

Practitioners are not frequently called upon to make a vaginal examination just before the obliteration of the cervix begins, that is, generally some days before active labour supervenes. But to this general statement, cases of placenta prævia form an exception. In them, it is well known that about the full time hæmorrhage commences quite unexpectedly. It in fact begins with the commencement of the dilatation of the internal os uteri, that is, with the commencement of the ordinarily painless contractions which precede active labour. If, at this time, examination is made, the cervix will be found quite softened and open inferiorly, but not yet shortened—a circumstance which I might illustrate abundantly by examples from my own experience, and from published records.

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* For evidence that this mode of examination was confided in by observers, see the work on pregnancy by Desormeaux and Dubois, *Dict. de Med.* tome xiv. p. 364.

The whole history of placenta prævia is inconsistent with the old views in regard to the cervix uteri. For if, after the middle of pregnancy, the neck be opened up and gradually developed from above downwards, hæmorrhage ought invariably to occur, and that to a great extent, before the full term is approached, which is not the case. Besides, there should, in cases that go to the full time, be found an atrophied portion of placenta corresponding with that extensive piece of new uterine wall produced by this supposed opening up of the cervix. With the views of Weitbrecht and Stoltz, on the other hand, the history of cases of placenta prævia quite accords. For while, in many cases, no bleeding occurs till near the full time, when the internal os uteri must begin to open ; in others, the bleedings which occur at intervals before reaching the full time admit of easy explanation. They may be due to the slight opening, to the extent of a line or less, which authors have described as occurring frequently months before the full time, or to vascular rupture, from other causes, at that unsupported point in the placenta which overhangs the internal os ; being then a consequence of the great tendency to miscarriage in placenta prævia—a tendency that is often in vain resisted.

On this subject I will not further enter, but refer to obstetric writers,* and especially a recent American author, Mr. Read (*American Journal of the Medical Sciences*, April 1858), for sufficient evidence of the extremely unsatisfactory nature of the notions now entertained on the subject. But while Mr. Read has done good service in exposing the intenibleness of the views now entertained, it is necessary to add that his own theory is, perhaps, the most intenible of all. For we find him arguing from the scarcely credible insertion of the placenta on the lowest part of the cervical cavity †—an argument, I need scarcely say, quite inapplicable to ordinary placenta prævia.

I have already said, that what is felt in careful vaginal examination is confirmed by the results of dissection. In the figures given, I have attempted to show the results of several dissections, and in them will be at once observed a marked uniformity of length of cervix at all periods of pregnancy. In this demonstration I shall do no more than explain the figures just referred to.

Figure 22 is an outline of the cervix of an unimpregnated female, after Farre, the author of the very complete article "Uterus," in the *Cyclopædia of Anatomy and Physiology;* an article which, I must add, contains views regarding the subject under discussion somewhat resembling those I have here given.

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* See some remarks by Dr. Alexander Milne, *Edinburgh Medical Journal*, November, 1867, p. 426.

† See Klob. *Path. Anat.* der *Weibl. Sex.* S. 538.

Figure 23 is an outline of the cervix uteri of a woman dying in the end of the third month of pregnancy. It is taken from the Atlas of M. Coste's work, entitled *Histoire générale et particulière du Développement des Corps Organisés*, etc.

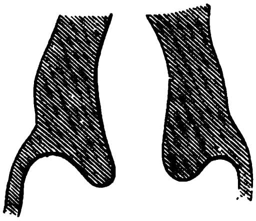

Fig. 22.

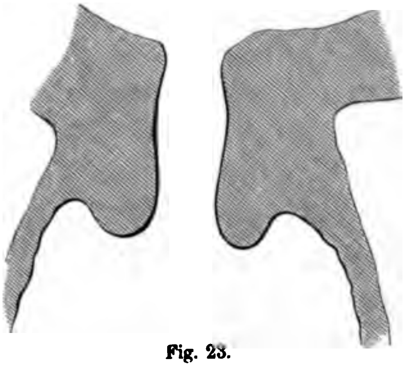

Fig. 23.

Figure 24 is an outline of the cervix uteri of about the same period of pregnancy as the last. It is taken from a dissection.

Figure 25 is an outline of the cervix uteri at the beginning of the fifth month, from the twenty-eighth plate of William Hunter's Atlas of the *Anatomy of the Gravid Uterus*—a work from which I might have drawn other materials for illustrating the present subject.

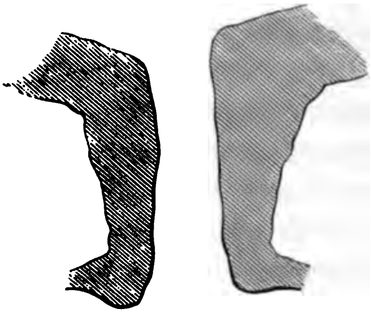

Fig. 24.

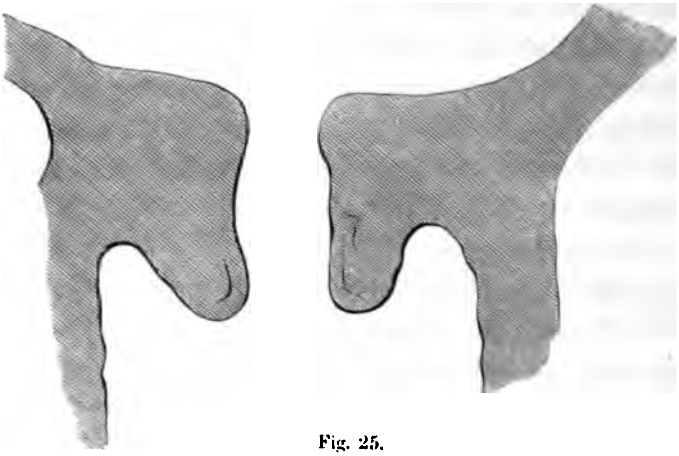

Fig. 25.

Figure 26 gives a tracing of a drawing made by Dr. Barnes *ad nat.*, from a preparation of the uterus of a young woman, who, in perfect health, destroyed herself by poison when at the beginning of the fifth month of pregnancy. The uterus was taken out entire, and injected by Mr. Lane. The drawing is from a section, of course after the injection. The preservation of the cervix and cervical cavity at this stage of gestation is complete.

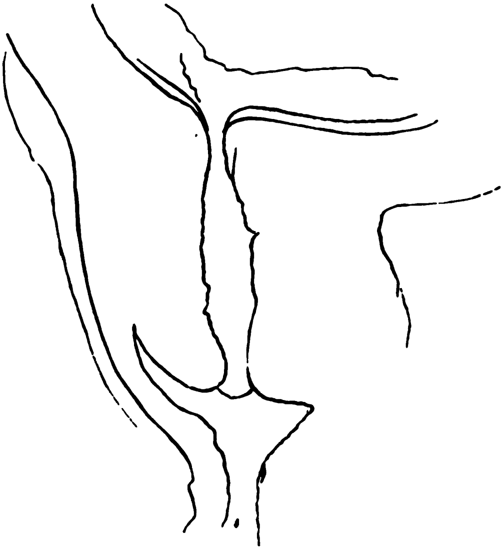

Fig. 26.

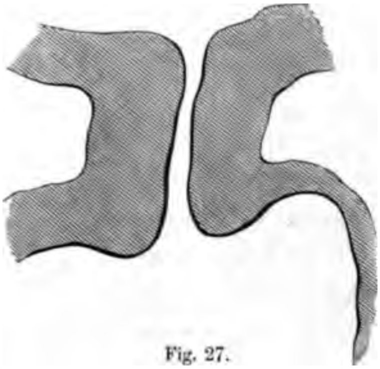

Fig. 27.

Figure 27 is an outline of the cervix uteri beyond the seventh month of pregnancy, from a dissection.

Figure 28 is a drawing obtained in the following way:—A woman labouring under typhus fever was admitted into the Royal Infirmary of Edinburgh under Dr. Warburton Begbie. He gave an unfavourable prognosis on account of the complication of the fever not only with pregnancy but also with insufficiency of the aortic valves. She died on the thirteenth day of the fever. Shortly after death the Cæsarean operation was performed, and a living child was born, which survived its mother about five hours. The statements of the patient and the appearance of the child showed that pregnancy was far advanced in the eighth month.

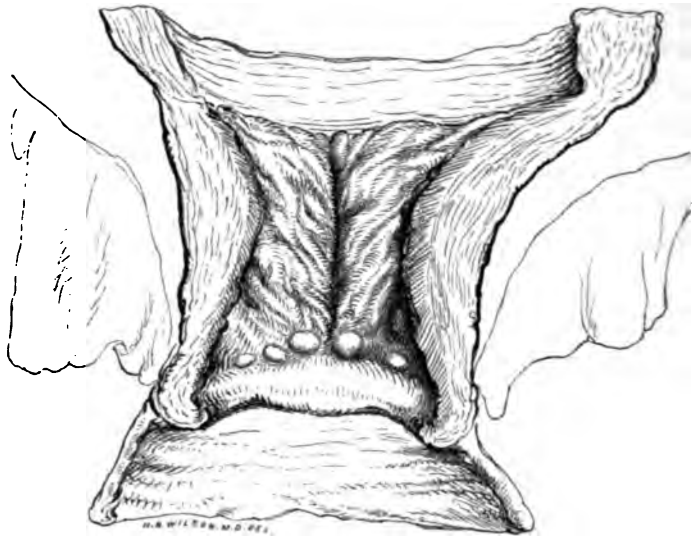

Fig. 28.

On the post-mortem examination, attention was directed to the condition of contraction of the uterus around the placenta, which was in part separated from its attachments, and expelled through the incision in the anterior uterine wall. The cervix uteri was examined by the finger in situ, but nothing could be thus decidedly made out. So glabrous was its surface, and so soft and unresisting its substance, that, had no further investigation taken place, one might well have concluded that it was obliterated and reduced to a mere os, and that Rœderer's erroneous descriptions were correct.

The entire uterus and neighbouring viscera were carefully removed and examined. The cervix was opened by a longitudinal incision in continuation of that made in the Cæsarean section. The appearances discovered are represented in the accompanying woodcut. The appearances which the part presents, as now preserved in suspension in a jar, very greatly resemble the picture in Rœderer's *Icones* of the same dissection of a woman in the sixth month of pregnancy. The cervix measured about an inch in length. It easily admitted the finger. Its tissue was greatly hypertrophied and completely softened in every part. The rugæ, especially the anterior and posterior columnæ, were greatly hypertrophied and prominent. The lower margin of the cavity of the cervix could be identified by the presence of a row of Nabothean follicles; the upper margin by the abrupt termination of the arbor vitæ and the expansion into the smooth-walled cavity of the uterus.*

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* I had, through the kindness of Dr. Fred. Steell, an inspection of

The majority of the preceding figures pretend to nothing more than giving outlines of sections of cervices made in various ways; some of them from old preparations, and without attempting to throw the parts into an attitude resembling what I might conceive to be natural. Some of them represent halves of bisected cervices, while in others the cervix has been forced open after one vertical incision and then drawn—a circumstance accounting to a certain extent for their differences from one another. But in the next figure I have attempted to give a drawing of all the appearances.

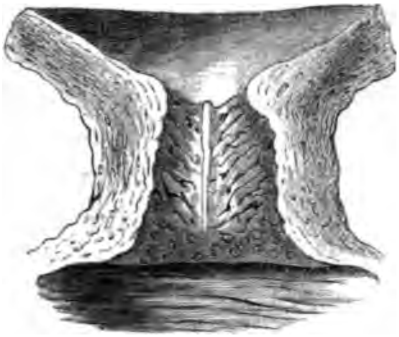

Fig. 29.

Figure 29 is a drawing of half of the bisected cervix uteri of a woman about the end of the eighth month of pregnancy, from a dissection. In this case, the internal os of the cervix was found open to the

the uterus, including the cervix 1¼ inches long, of a woman who died near the eighth month of pregnancy from croup, and on whom Cæsarean section had been performed. The cervix closely resembled that given in figure 29.

extent of somewhat more than a line. Above it the expanded cavity of the uterus was covered by its smooth mucous membrane. Below it began the rugæ of the cervix, having a very characteristic appearance. The external os of the cervix is seen to be widely dilated.

In the uteri which I have examined, or of which I have seen careful drawings, the cervix presents some variety in measured length, the convenient term of an inch being sometimes exceeded, in others not reached; and in a part which is soft, as this, it must be remembered that slight variations in length may be recorded by the same observer, according as the part is placed and handled. In the figures in the woodcuts here supplied, the general length is about an inch.

There are two anatomical details which are observed in the dissection of gravid uteri at all periods of pregnancy, and which appear to me to afford strong evidence of the correctness of the views enunciated by Weitbrecht. These points relate to the situation at which the uterine artery approaches the cervix, and to the situation of the broad ligaments upon the side of the womb. The condition of the womb in respect of both these particulars remains during pregnancy nearly the same as in the unimpregnated state. Were the neck of the womb developed, as most obstetric writers assert, then it would naturally be expected or would necessarily follow that, with the growth and expansion of the cervix into the lower part of the great uterine cavity, the uterine arteries and the lower parts of the broad ligaments would be elevated, accompanying the growing adjacent parts, and that the artery would appear to rise to some part of the expanded uterus. The fact that such changes are not observed lends an anatomical argument, were any such necessary, in favour of the views which we have laboured to establish by anatomical facts.

I have refrained from the appearance of greater exactness, being sorry I cannot pretend to have reached it. I think, however, that it is very desirable that a series of careful and minute measurements of this part be made and recorded.

The capacity of the cervical cavity becomes gradually greater as pregnancy advances; and this is effected by an increase of its diameters, or breadth, advancing from below upwards, that is, from the external to the internal os of the cervix.

As Cazeaux has well pointed out, this order of sequent changes is, in primiparæ, often if not generally perverted, in so far as the external os often remains little increased, while the cavity of the cervix above it is dilated.

I wish to say nothing in regard to asserted changes in the shape of the os uteri, believing that they are subjects for further investigations.*

The length of the vaginal portion of the cervix, or the amount of its projection into the vaginal cavity, generally diminishes as the uterus rises into the cavity of the abdomen.

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* Stoltz, *ibid.*, describes them on the authority of Stein.

This is far from being a constant phenomenon of pregnancy. When it does occur, it is one of the causes of that error as to the shortening of the cervix into which accoucheurs generally have been led. It is not very easy to account for it, unless a sufficient explanation is to be found in the elevation of the cervix, when the womb rises into the abdominal cavity producing a sort of dragging on the vagina, which consequently descends so as to appear to be inserted close on the margin of the os externum. And there can be no doubt that this change takes place in many instances of elevation of the uterus in disease. The opposite result, also, is well known to be produced by depression of the uterus. And even in early pregnancy, when the uterus is slightly depressed in the pelvis, this change has led Boivin and Filugelli to describe the cervix uteri as really lengthened in these months.

I am unable to apply actual measurements, the best criterion, to this point; and I believe it quite possible that such, when made, may show that this apparent shortening of the vaginal portion of the cervix is merely the result of the increased bulk and roundness combined with softness. MM. Dubois and Pajot have carefully discussed this point in the second part of their work on midwifery.

The softening of the cervix uteri, already commenced superficially during the menstruation preceding conception, continues and extends more deeply into the substance of the cervix as pregnancy advances, and the process is generally completed before the end of utero-gestation. This softening is accompanied by a considerable increase of bulk.

As I have, on this head, nothing to add to the observations of others, I deem it a work of supererogation to enlarge upon it.

In vaginal examinations during life, after the middle of pregnancy, the finger of the obstetrician feels the uterine cervix as if it were gradually shortened according as pregnancy still further advanced.

This is merely a statement explaining how obstetricians have hitherto so much erred regarding this subject. The error might always have been avoided if care had been taken to estimate the length of the cervical cavity, which, with its mucous membrane, forms the only characteristic part of the cervix. The finger examining without passing into the cervical cavity, is led into error by the softness of the part and its easy compressibility. M. Cazeaux has ascribed the erroneous impressions, partly to the finger while feeling the external surface of the cervix being arrested by the vaginal reflexion. But, in my opinion, this cannot often be the case. For the vagina is, at all periods of pregnancy, sufficiently easily displaced for the purposes of this examination, and the exploring finger is arrested by the tumid uterine body, not by the vagina refusing to be displaced. This is easily understood by a reference to figures 22 and 29. In the former, it will be seen that the external surface of the cervix is nearly parallel to the internal, and has nothing overhanging its upper extremity by which the finger may be opposed; while in the latter, the external surface of the cervix is seen to be at least virtually much shortened by the swelling and distension of the wall of the lower part of the body of the uterus, which also forms a firm obstacle to the upward thrust of the examining finger.

[illegible]

[illegible] CASES [illegible]

[illegible] discharges [illegible] All [illegible] disagreeable. [illegible] but [illegible]

[illegible] vaginal discharges [illegible] in utero or [illegible] un- [illegible] lochia: the [illegible] discharges: the [illegible] at different [illegible] parts [illegible] the retention of [illegible] membrane: cancer of the vagina or uterus; extra-uterine pregnancy; disintegrating fibrous tumour; abscess of the genital organs, or in the pelvis; and thrombus or haematocele in the same parts. These [illegible] not invariably, or under all circumstances, [illegible] but they all frequently do so; and it [illegible] tents and pessaries often rapidly [illegible] discharges, as well as anything introduced from without that will decompose or lead to retention of matters that readily undergo this change.

Cases of retention of the entire ovum, at or near the full time, and cases of retention and absorption of the placenta, or of portions of it, have been recorded. Those to which I now attract attention are more interesting, and less familiar to the profession. It is well known to obstetricians that part of an ovum may be retained and cause putrid discharges, which are sometimes accompanied with danger to life, and which generally disappear in a shorter or a longer time; but the following cases have points of interest which distinguish them from ordinary accidents, and give them interest and importance.

Writing on the subject of abortion, in his work on Midwifery, Dr. Burns* says—"While part of the ovum is left, or the whole of the secundines are retained for a considerable time, we have another danger besides hæmorrhage; for within a few days putrefaction comes on, and much irritation is given to the system, until the fetid substance is expelled. Sometimes, if gestation have not been far advanced, or the piece which is left be not very large, it continues to come away in small bits for many months, during the whole of which time the woman is languid, hysterical, and subject to irregular sanguineous discharges, or often to obstruction. But more frequently the symptoms are very acute; we have loss of appetite; prostration of strength; tumid or tender belly; frequent, small, and sharp pulse; hot and parched state of the skin of the hands and feet; nocturnal sweats, and various hysterical symptoms. The discharge from the vagina is abominably fetid, and hæmorrhage sometimes occurs to a violent degree."

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* *Principles*, tenth edition, p. 334.

Some of the cases to be recorded illustrate consequences of imperfect abortion to which Dr. Burns does not make any reference. They show that part of an early ovum may be retained without any urgent symptoms for several months. Like Dr. Burns, Dr. Ramsbotham[*] believes that retained parts of placenta begin to decompose and be putrid very early, and at farthest within a period of some days. Dr. Ramsbotham, indeed, considers it worthy of remark that he has "known a number of instances in which the placenta was not thrown off for many days after the young embryon had passed, and yet it did not possess in any degree a putrid odour." The following cases show that months may elapse before the sign of putridity is observed. This lapse of time between the incomplete abortion and the supervention of serious symptoms especially of putrid discharge, together with the general teaching on this subject, illustrated from the justly popular works of Burns and Ramsbotham, sufficiently account for the circumstance that the most interesting of the cases now recorded were inaccurately diagnosed by the physicians under whose care they at first happened to be.

The absence of fetor in some examples of imperfectly completed abortion is very remarkable, and almost unaccountable; and this absence of fetor renders such cases less likely to be correctly diagnosed and treated, and makes them, in this respect, of much importance. The presence of fetor, indeed, ought soon to attract attention, and lead to careful investigation of the case in which it occurs; but, in its absence, there may be no special symptom to lead the practitioner to suspect that the case under his care is one having any character to distinguish it from more ordinary uterine ailments. The following case of retention of an ovum for seven months is a remarkable instance of the absence of fetor, or at least, of its presence to so slight an extent, in a scrupulously cleanly lady, as never to be noticed by herself or by her husband, himself a physician. I would, indeed, have it understood that by absence of fetor I mean its absolute absence, or its being such as not to attract the attention of careful, cleanly individuals.

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[*] *Obstetric Medicine and Surgery.* Third edition, p. 666.

CASE I.—Mrs. —— was delivered of her second child fourteen months before consulting me. When the child was about seven months old she believed herself to be again pregnant, and therefore gave up suckling. But she soon resigned the notion of her being pregnant, all symptoms of that condition gradually disappearing. After this time her uterine health became impaired. She suffered from weight in the uterine region, slight bearing down, occasional irritability of bladder, irregular action of the bowels, occasional disorder of the stomach, and even sickness.

She soon began to have a constant leucorrhœa of a brownish colour, and bright bloody discharges were never absent above a week, the loss in this way being occasionally somewhat profuse. Having these symptoms, she made the long journey from India to Edinburgh. Soon after her arrival here a severe metrorrhagia occurred, and I was called to see her. The uterus was found enlarged, its cavity being also larger than natural. The cervix was healthy, but preternaturally open, the end of the finger being easily passed just within the circle of the os externum. The case was diagnosed as one of retention of some of the remains of an ovum which had produced, for a time, the symptoms of pregnancy, had ceased to live, but had never been entirely expelled. A sponge-tent was inserted into the cervix. On the day following its introduction both the tent and the blanched ovum were found in the vagina. They were very fetid, as was to be expected, from the use of the tent. The ovum was a placental mass an inch and a half broad and above half-an-inch thick. On opening the bag of membranes a few drops of dirty brownish fluid escaped; no remains of an embryo or cord were discoverable, and the ovum perhaps never contained any, being addled from the beginning. The fœtal surface of the placental mass presented prominences distended with bloody fluid, such as are often seen in similar circumstances. The patient soon recovered, and after aborting twice, bore a healthy child at the full time.

Case II.—A case somewhat similar to the preceding occurred to me several years ago. The patient was a soldier's wife who had miscarried at about the end of the third month of pregnancy. She was attended by a woman who told her that all had come away right. But from that time till I saw her, ten weeks after the abortion, she never ceased to have bloody discharge. At this time the placenta came away. It was not fetid, but had undergone peculiar changes which I do not here describe: only it was œdematous, and its fœtal surface was covered by numerous distended bladders of chorion of the size of half of a boy's marble, and filled with bloody fluid.

I have met with several cases like this last which I need not record, being remarkable only for the absence of fetor in the discharges.

Speaking of a case of Dr. Ogle's, in which absence of putrescence was remarkable, Dr. M'Clintock says—"This recalls to my mind the case of a lady I attended in a four months' miscarriage, who was just recovering, under the care of Dr. Geoghegan, from a long and dangerous fever. The fœtus slipped away with little pain, but the placenta and membranes remained incarcerated in the uterus for fourteen days; and yet, none of these secundines, when discharged, exhibited the slightest trace of decomposition, or exhaled any unpleasant odour."*

On retention of the placenta without decomposition, Hegar makes the following remarks†—"The separation and casting out of the placenta, or of remains of it, may take place still later, even after many months, without any other appearances than the retarded involution of the uterus, and its consequences, and bleedings. Slight traces of decomposition of the placenta and its results are indeed sometimes met with, yet they are not met with in all cases. In such cases the placenta must necessarily have remained still in intimate union with the uterine wall, and, in addition, the vascular connections must have remained intact, because otherwise their nutrition is inconceivable. This kind of termination is found especially after abortions, but examples also occur when the placenta, or remains of it, have been evacuated long after delivery at the full time, without any decomposing process having begun."

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* *Clinical Memoirs on Diseases of Women*, p. 193.

† *Placentarretention*, S. 85.

The case which I have next to record differs from the two former in the circumstance of the presence of a very profuse and intensely fetid brownish discharge evidently containing pus, and in the discovery of decomposing structures. Besides, it was erroneously diagnosed and treated as a case of ulceration of the uterus. The speculum and caustic had been diligently used, this treatment being the resort, in almost all uterine cases, of many ignorant and not a few careless practitioners. On this I say no more at present, because it would be out of place to enter now on the many gross abuses of a plan of treatment sometimes found very useful.

It should be remarked that in Cases III. and V.

fetor did not commence till after the disease had lasted for a long time. I am unable to give a quite satisfactory reason for the delay of the appearance of the putrid odour; but the long delay in two of the following cases brings the two preceding cases into the same category with them, it being almost certain that in them also, had the retained structures not been completely discharged, intense fetor would soon have made its appearance. The following appears the most probable method of accounting for the delayed putrescence (as in Case V.): the ovum dying, or by some morbid process becoming disconnected from the healthy decidua, may pass away entire and without decidual structures, or break down and be discharged; or it may be accompanied by only a portion of the decidua. In such cases the remaining decidua may be supposed to retain its uterine connections for an indefinite time, say weeks or months, and then being thrown off, may be retained in utero, and at last become putrid. When the cause of abortion is disease of the decidua, especially if there be hæmorrhage beneath it, or when uterine contraction or spasm is strong, then persistent adhesion and retention will be evidently less likely to occur than when the decidua is healthy, and uterine contractions are inconsiderable. This mode of explaining the delay of putrescence is scarcely applicable to instances like Case I.; for in such it is not decidual but ovuline, and especially placental structures, that are not putrid. And in such cases, while a prolonged vegetable life can scarcely be imagined to continue without growth or hydatigenous or other degeneration, yet it is easy to suggest that a kind of decomposition without putridity may explain the phenomenon.

CASE III.—Mrs. B., æt. 25, was mother of four children, the youngest of whom was born nine months before her admission into the Infirmary. After its birth she was for three days much troubled with afterpains, but made, upon the whole, a good recovery. A month after her delivery she began to have severe pain in the lower part of the belly, and in the back and thighs, with bearing down, and at the same time a leucorrhœal discharge made its appearance. She was so ill as to require to be constantly in bed or on a sofa. Four months after delivery she menstruated, and this change thereafter returned nearly regularly. At these times the loss of blood was so severe that her physician plugged the vagina each time, and the discharge lasted long—on one occasion for nearly three weeks. Two months before admission the discharge became more brown in colour and extremely fetid.

When admitted into the Royal Infirmary she had the symptoms above mentioned. The discharge was profuse, brownish, purulent, and very fetid. The uterus itself was not remarkably tender, mobile, and without surrounding induration. It was much enlarged and capacious, its cavity being large and having a length of about three and a half inches. The cervix uteri had a healthy surface, but the os was unnaturally patent.

Sponge-tents were used to effect dilatation of the cervix, and the interior of the uterus was explored with a long polypus-forceps. Shreds of membrane and little masses of decomposing tissue were brought away, but it was found impossible to identify the special structure. The uterus was now repeatedly washed out by injecting freely a stream of warm water through a double catheter, such as is used in injecting the bladder. Numerous shreds of tissue came away. Ten days after this treatment was commenced the fetid discharge had disappeared; the uterus was reduced to its natural dimensions, and the woman soon left the hospital quite well.

Even in women carefully delivered at the full time, accidents may happen analogous to those illustrated in the cases already mentioned. In most such cases, when a portion of placenta, or of membrane, or of both, is left adherent to the uterine wall, separation and extrusion or decomposition and discharge of the abnormally persistent parts is effected soon after delivery,—that is, within four weeks. I do not discuss the question of resorption of such parts, believing that such an event is, so far as fœtal structures are concerned, physiologically impossible. But there is no doubt that parts of the placenta may be retained undecomposed for a considerable period, and then break down into fetid discharge, or be expelled entire. The following case will illustrate this to some extent; it is interesting, because an able and careful practitioner and a good monthly nurse both satisfied themselves at the time of the confinement that the afterbirth was delivered entire and not disrupted. There is a very important class of cases of menorrhagia or periodical monthly floodings occurring after delivery, from which this description of case is easily distinguished and separated by the two circumstances, that the cause of the flooding was a retained lump of placenta, and that the flooding took place only once, the cause of the affection being removed.

Case IV.—Mrs. P., multipara, was easily delivered of a child at the full time. After its birth some hæmorrhage flowed, but it was arrested on the removal of the afterbirth. She made a slow but good recovery; and the discharges were reduced to a very insignificant amount. Four weeks after delivery, on attempting to get up, hæmorrhage took place, and on making a similar attempt next day, blood flowed still more abundantly. Arriving at the bed-side, I made an examination, and discovered a firm mass dilating the upper part of the cervix uteri. This I proceeded to seize with a forceps, intending to remove it by any means that might be necessary. It was easily brought away, and found to be a placental mass of the size of a large walnut. It was not in the least degree fetid. After this the hæmorrhage ceased and convalescence was soon perfect.

It is generally believed that putridity always occurs very soon in cases of retention of a portion of the fully-developed placenta. Dr. Ramsbotham remarks "that the placenta of an early ovum does not always putrefy when left in the uterus, although that of a child at full time, under the same circumstances, invariably takes on the putrefactive process, and that within thirty or forty hours." Case IV., just related, sufficiently proves the possibility of entire absence of putridity for a whole month.

I am not disposed to make any marked distinction, in respect of putrescence, between retentions differing only as to the period of pregnancy which the ovum had reached; and in the following passage Cazeaux blends the description of both. Speaking of tardy expulsion of the placenta, when it may take even ten days to become completely separated, he remarks,—"On examining the placenta, it is found to be in no way altered, it offers no bad odour, and although it has sojourned several days, several weeks, sometimes even several months in the uterine cavity after the expulsion of the infant, it is as fresh as if the child had only been newly born. The integrity of its vascular connections had maintained its life, and accounts for the innocence of the prolonged retention. This is what I had occasion to observe in a young woman, who, twenty-four days previously, had had a miscarriage of three and a half months. The afterbirth had remained from this time in the uterine cavity, and a serious hæmorrhage having come on in consequence of the separation of the placenta, I was obliged to extract it artificially. It was, however, already engaged in the cervix, and its extraction presented no serious difficulty; but the extreme feebleness of the patient brooked no temporising. It offered no appearance of putrefaction. Unfortunately the slowness with which sometimes the separation of the placenta takes place may prolong the hæmorrhage, and become also the cause of another accident. When, indeed, a cotyledon is thus detached, it ceases to participate in the circulation of the adherent parts, and lies hanging in the cavity of the uterus. After a certain time, it is separated from the rest of the placenta, and if its volume or the contraction of the os prevent its immediate exit, it putrefies, and may become the cause of some of the accidents mentioned above. Most frequently, however, its expulsion has not to be waited for long, or at least the physician judges it proper to effect its extraction; but what it is impossible to avoid is the hæmorrhages, which, returning whenever a renewed partial detachment takes place, end in greatly weakening the patient and even compromising her existence."*

But the most common and easily accounted for cause of fetid discharges from imperfect abortion, is retention of masses of decidual membrane. This occurs chiefly in the early months of pregnancy; for as this state advances beyond the third month, the decidua is rapidly thinned and atrophied as the uterus is distended, and does not anywhere present any mass, unless indeed it be at the edge of the placenta.

The advancement of the pathology of abortion, a subject little understood, will surely throw much light on the subject-matter of this paper. At present I merely remark, that in abortion it sometimes happens that the entire double layer of decidua is discharged with the ovum: in this case the abortion may be truly called complete. It also sometimes happens that the ovum alone is discharged, unaccompanied by any decidual structures; and in such cases this incomplete abortion is followed, after a few hours, or even a day or two, by the expulsion of the remaining decidual masses. Occasionally no such decidual masses are discharged as masses, and yet recovery is undisturbed; and in cases of this kind the persistent decidual membrane must either disintegrate rapidly and come away imperceptibly in the discharges, or, maintaining its uterine connexions, the membrane may slowly exfoliate and atrophy in like manner as it does after ordinary menstruation. But it also, though rarely, happens, that the decidual masses are retained for many weeks undecomposed [perhaps adherent to the uterus], and then become separated, putrefy, and cause fetid discharges, until they are expelled. In the following case, this series of events occurred; and it is to be noted that fetor was not observed till five months after the end of the first part of the miscarriage.

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* *Traité de l'Art des Accouchements*, p. 925.

Case V.—Mrs. M., æt. 36, had borne several children. Her youngest child was six years old. Six months before admission to the hospital, she miscarried at about the end of the third month of pregnancy. In about a fortnight thereafter she felt quite well, having only a slight yellowish discharge. But, beginning to walk, she found the discharge become bloody, and it continued so, more or less, till she came to the Infirmary. In the fifth month after the miscarriage, the discharge became much more profuse, occasionally coming away in clots. Before the expulsion of clots she had always much abdominal pain. Shortly after this, the discharge became fetid, and the stench about her person was sometimes so great that her children could not abide in the same room with her.

On examination there was observed a very fetid, grumous, bloody, and purulent discharge, containing some shreds of animal tissue. In the vagina was found a rounded decomposing mass of decidua of the size of an almond. Its rounded shape differed from that of the irregular lacerated piece it had originally been, just as a water-worn boulder differs from an unaltered angular fragment. Another piece of decidua like the former was soon afterwards discharged. The os uteri was morbidly patent, and the uterine cavity was three-fourths of an inch longer than is normal. The woman was in a very bad and weak state of health generally.

As fetid discharge still continued, the os uteri was dilated and the uterine cavity washed out with warm water, as was done in Case III. In three days thereafter the fetor was no longer observed. The discharge rapidly diminished; the general health improved; and the woman soon left the hospital cured.

I might, from my note-book, here give other cases, exactly similar to that last described. But it is unnecessary so to load my pages.

It is to be remarked that all the cases here recorded were characterised by more or less of the local pains of uterine ailment, by the presence of discharges which were either purulent or bloody, by occasional metrorrhagia, by patency of the os uteri, and a hypertrophied condition or a state of imperfect involution of the uterus. Besides, after the lapse of a considerable time, in Cases III. and V., the discharges assumed an intense fetor.

The importance of this class of cases need not be insisted on. By means, the use of which is illustrated in Cases I. III. and V. the diagnosis will be easily made if the practitioner only suspects the true nature of the case, and is thus induced to investigate it.

For the cure, nothing is required but the complete evacuation of the offending structures from the interior of the uterus. This is effected by the use of the sponge-tent, or tangle-tent, the polypus-forceps, and the double catheter injection apparatus.

The size of the decidual masses seen in such cases, might cause some doubt as to their nature, were it not known from the observations of Hegar, Huter, and Müller, that around the placenta the membrane is specially liable to hypertrophy and retention.

CHAPTER IX.

ON IMPERFECT DEVELOPMENT AND HYPERTROPHY OF THE DECIDUA.

THERE is no field of observation more needing the cultivation of the obstetrician than the morbid anatomy of the decidua. The backward state of this department of obstetrical pathology, and the consequent demand and openness for cultivation, probably depend upon the circumstance that the true normal anatomy and physiology of this structure is only now becoming thoroughly known.

In a paper "On some Results of Imperfect Deliverance in Abortion or Labour," published in the *Edinburgh Medical Journal* in January 1863, I made some remarks on the occurrence of hypertrophy of the decidua, and I now return to the subject with a view to enlarge them and add to them.

In 1861, Virchow described to the Obstetrical Society of Berlin a case of a peculiar kind of hypertrophy of the decidua, which he was inclined to ascribe to syphilitic poisoning. In this case* the simple hypertrophy of the whole membrane was not very great. Virchow regarded the disease as the result of an endometritis papulosa et tuberosa, for the inner surface of the decidua had on it many rounded, somewhat nipple-like elevations of various sizes, such as are sometimes produced on this membrane when inflamed in the unimpregnated state. The disease produced abortion.

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* *Archiv für Pathol. Anat. und Phys.*, 1861. Bd. xxi. S. 118.

In December of the same year, Strassmann showed to the same Society a very fine specimen of what he calls a peculiar hypertrophy of the decidua. In this case, of which, as of Virchow's, there is a drawing given, the hypertrophy was very great. It also presented the projections on its inner surface seen in Virchow's case; but Strassmann saw no reason to suppose that the disease had a syphilitic origin.

Of this peculiar kind of hypertrophy of the mucous membrane of the body of the uterus in the pregnant state, I have seen no example; but I possess a characteristic specimen of it in an unimpregnated uterus. Strassmann regards it as rare, and I have at present no reason to doubt the accuracy of his opinion. But, in my paper of 1863, I had not in view this special kind, but a simple hypertrophy of the decidua; and I am sure that this simple hypertrophy is far from being very uncommon. I have met with many examples of it.

I here give a sketch of the mass discharged in a case of this kind. The small ovum inserted in the fundal portion of the decidua is seen surrounded by the exaggerated growth of the membrane. The patient who gave birth to this was the mother of a large family, and thought herself nearly three months gone in pregnancy.

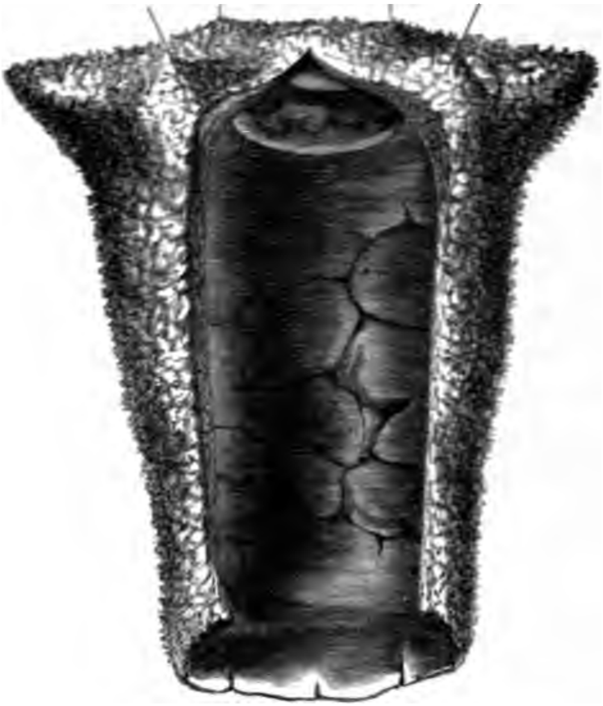

Fig. 30.

In another case the decidua was discharged in various masses, and its hypertrophy was as great in thickness as in superficial extent. In both it was very great. The fœtus in this case was arrived at the third month of gestation, and it had a spina bifida.

Fig. 30. Showing a hypertrophied decidua. The ovum covered by decidua reflexa is seen adhering to the fundal portion of decidua vera. One wall of the decidua vera is laid open to show the interior cavity.

I have notes of another extreme case of this hypertrophy, in which the inner surface of the decidua was covered at parts by a layer of lymph, croup-like. In this case the ovum indicated the advance of pregnancy far into the second month.

In most of these cases the decidua has presented to the eye a natural appearance. In some, it has had a want of uniform tinge of colour on the surface, some parts being paler than natural. In all the specimens which I have had examined, the membrane has been found in a state of fatty degeneration; and in one case, when there was a mottling of the surface with pale spots, it was found by Dr. Grainger Stewart that in the paler parts fatty degeneration of the tissue was more advanced than in the redder parts.

This hypertrophy of the decidua is observed, as already shown, in two different sets of circumstances. In the one it is in extent and thickness quite out of proportion to the small ovum; and in such, the history generally makes it probable that the ovum being or becoming addled or otherwise diseased, has ceased to grow, but has retained its vitality, and the decidua has continued to grow. Specimens of this kind of hypertrophy are the most frequent. The woman, often a person of experience, reports herself as six weeks or two or three months gone in pregnancy; she aborts and discharges an immense decidual mass, having an ovum not bigger than a boy's marble in one corner of it. In another kind, the ovum has grown to a size proportionate to the probable duration of pregnancy, but the decidua is even larger than what naturally appertains to such an ovum.

With a view to easy comparison of the hypertrophied decidua, above depicted, with one of natural dimensions, I here give a sketch taken from Coste's great work.* It is seen to contain an ovum covered by reflexa, as in figure 30, and of about the same size. Coste describes the pregnancy as being of about 40 days. It is to be observed that, for comparison with the two other figures in this chapter, this drawing of Coste's should be reduced one-half, because the decidua is opened out and its whole inner surface exposed, where-

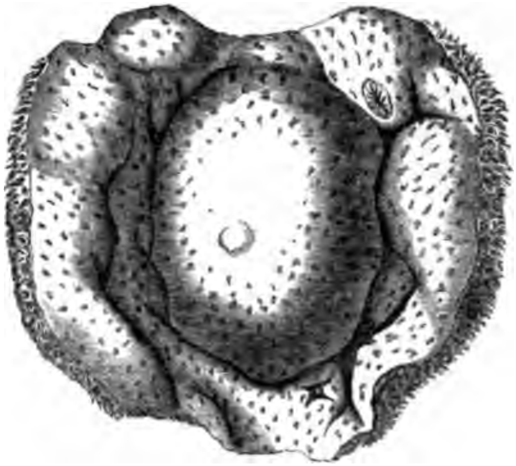

Fig. 31.

Fig. 31. A healthy decidua. The ovum, covered by decidua reflexa, projects from the centre of the mass. Only the internal surface of the decidua vera is seen.

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* *Histoire du Développement*, etc. Atlas, Plate V^b, Fig. 4.

as in the others only the external surface, or chiefly this surface, is seen.

Imperfect development of the decidua has not so often come under my notice in characteristic cases as hypertrophy. It is easy to conjecture that deficient hypertrophy or imperfect decidual development may be a cause of sterility, and this Bischoff[*] is said to have particularly pointed out. In the well-marked case of which I propose to give a few particulars, sterility was much complained of. To be sure, the sterility was not absolute, but it was as good as absolute to the husband and wife, an early abortion being the only result of the pregnancy which had already occurred. The lady who was the subject of it had been long married. She had an imperfectly developed, undersized, uterus. She had already had a miscarriage, of which no exact history could be got. After having gone exactly three months in pregnancy, as she calculated in the usual way, she gave birth to an abortion, with ordinary attendant symptoms. The ovum was a simple bag sparsely covered with chorionic villi, except where it was in contact with the decidua. The actual size and general appearance are given in the woodcut. Its extreme smallness will be evident to every obstetrician, and any reader may have some estimate of it by comparing it with the figure from Coste, given in figure 31. Further, it has to be remarked that along with the decidua a thin layer of the mucous membrane of the cervix has been stripped off in the abortion, and this gives it a larg appearance than it would have had were nothing fi ured but the decidua.

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[*] See Kussmaul. *Mangel*, etc., *der Uterus*. S. 287.

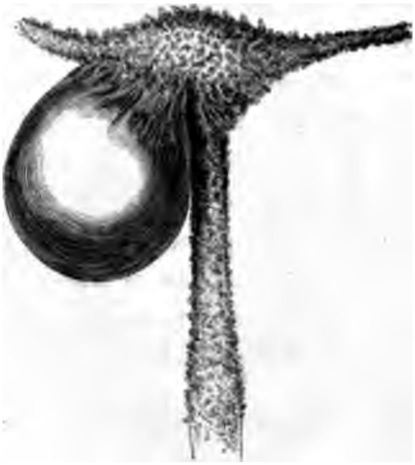

Fig. 32.

Students of this subject will find it elaborately ar learnedly discussed by Dr. Alfred Hegar in the su plement to the twenty-first volume (1863) of tl *Monatsschrift für Gebürtskunde.*

Fig. 32. Showing the external surface of the decidua vera, and t ovum ; the decidua being imperfectly developed.

PART IV.

ON SOME TOPICS IN NATURAL AND MORBID PARTURITION.

PART IV.

ON SOME TOPICS IN NATURAL AND MORBID PARTURITION.

CHAPTER I.*

A CONTRIBUTION TO THE DYNAMICS OF LABOUR. THE POWER EXERTED IN ORDINARY LABOURS.

THE dynamics of natural labour have been the field of very little successful study or investigation. I purpose, first, to show what amount of pressure per square inch is sustained by the ovum in the easiest class of natural labours, and thence to estimate the propelling power exerted in such cases.

It is well known that natural births are ever and anon occurring, in which the ovum is expelled whole, the membranes containing the liquor amnii continuing entire. Into this category many more cases would enter, were it not a generally-followed rule for the attendant to rupture the bag should it advance entire as far as the external parts. Again, as Dr. Poppel † has pointed out, the attentive observer of a series of easy natural labours has no difficulty in arriving at the

* From *Trans. R.S.E.* 1867. † *Monatsschr. für Geb.* 1863.

conclusion, that in not a few cases the same force which ruptures the bag of membranes is able to, and actually does, complete the delivery.

In all such cases, the strength of the membranes to resist impending rupture measures the force exerted in the process of parturition. When the bag is produced without laceration, its strength exceeds, certainly only to a small amount, the power of the labour. When the bag is ruptured at a very advanced stage of labour, as not rarely happens, its strength exceeds the power of labour exerted up till the time of its rupture. When the bag is ruptured by pains, which, without probably increasing in strength, rapidly and easily terminate the process, then the power of labour is only a little greater than the estimate, founded on the strength of the membranes, would indicate.

The strength of the membranes is thus shown to give us a means of ascertaining the power of labour in the easiest class of natural cases.

It might be suggested that, in cases of persistent membranes, they were specially and unnaturally strong. My own experience lends no support to such a notion. Besides, so far as I know, no obstetrician has used the only means of verifying such a supposition—means such as are exemplified in the experiments to be hereafter related. Obstetricians have judged of the strength of membranes to resist a bursting force by their united thickness, or other less definite qualities, which form no criterion. It is not uncommon to read of the bag being strengthened by decidua; and that such thickening may be a source of strength is a common opinion; but as the decidua is far weaker and less extensible than the other membranes, the opinion is merely a natural delusion. In like manner, it is common to hear tenuity of the membranes spoken of as if this quality necessarily indicated weakness, an opinion which also is erroneous.

Experiments at once show that thickness of the membranes is no indication of strength. They also at once show that, for the special purposes of this paper, the amniotic membrane, being the strongest, alone requires to be observed. Long before the amnion is burst, the decidua and chorion have generally given way, and ceased to support the persistent amniotic membrane. The decidual membrane generally gives way first, under a bursting pressure applied to all three membranes. It sometimes does so, in experiments, with a sound as of a gentle fillip. Occasionally it bursts simultaneously with the chorion; and occasionally, but rarely, all three membranes burst at once. The decidua has been found, in my experiments, to burst at a tension of ·35 lb. per linear inch, corresponding under the circumstances supposed to exist in actual labour to a forward pressure of nearly 5 lbs., thus exhibiting an amount of strength quite unexpected.

As a general statement, it may be said that the chorion behaves like the decidua. It is of more uniform strength than the decidual membrane, and is only a little stronger, the average tensile strength being ·62 lbs. per linear inch, corresponding to a propelling power in labour of nearly 9 lbs. [In taking these averages, experiment 25 is omitted, because its exceptional value indicates almost certainly a mistake.]

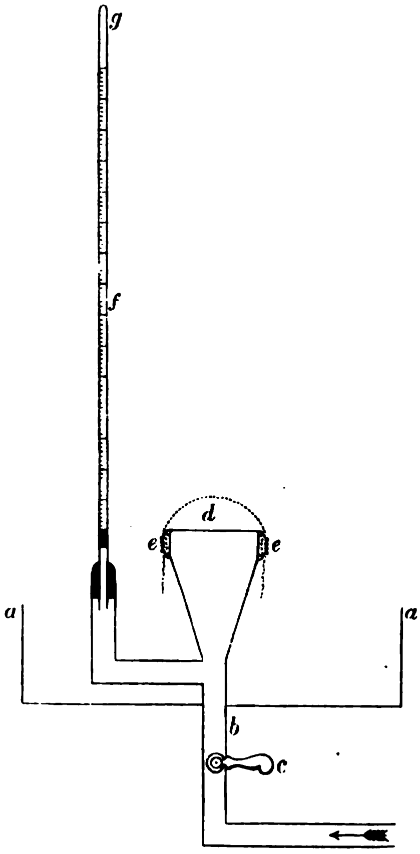

Fig. 33.

The strength of the fœtal membranes lies in the innermost sac, in the amniotic membrane, which appears the thinnest and most delicate of all. To try the strength of it, as well as of the others, I made numerous experiments in the manner to be described:—They were all performed in the laboratory, and with the apparatus of Professor Tait, to whose knowledge and skill I am indebted for their value and accuracy. The apparatus used was connected with a pipe *b* in the bottom of an open cistern *aa* (Fig. 33). Into this pipe water,* under high pressure, of which there was a convenient supply, could be admitted gradually by a cock *c*. The apparatus expanded upwards from the pipe to its mouth *d*. In one apparatus used this mouth had an external diameter of 3·35 inches, in the other it had an external diameter of only 2·25 inches. Over the mouth of the apparatus the membranes experimented on were placed, and tied on by a waxed hempen cord, around a broad rim *ee*, immediately beneath the mouth. That the apparatus acted in a fair and satisfactory manner was evident, from the observation that, in almost all the trials, the membrane tested did not burst where it touched the instrument, but in an arc of a circle crossing over the bulged-out membrane; or rarely, in a starlike manner. Connected, by a hollow arm, with the apparatus was a vertical glass tube *g*, with scale *f* of inches and tenths of inches. This tube contained a long column of air, confined in it by a short column of mercury. The rise of the column of mercury compressing the air in the tube indicated the degree of pressure applied to the internal surface of the membrane fixed over the mouth *d* of the conical vessel. Besides my own supply, I was kindly provided with fresh membranes by Dr. Linton and Mr. Vacher of the Royal Maternity Hospital.

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* Water is preferable in these experiments to air, because, when it is employed, there is less violent action at the bursting of the membrane.

The following table gives, in a categorical form, a narration of each of 100 experiments, as well as the chief calculations founded upon the data obtained from them. The first column gives the number of the trial. The second column gives the number of the set of membranes tested; and it will be seen that, generally, several experiments were made with the same membranes. The third gives the length of continuance of labour till the time when the membranes were ruptured. The fourth column gives the duration of the first stage of labour. The fifth gives the duration of the second stage of labour. The sixth column contains the state of the os uteri at the time of the rupture of the membranes. The seventh states the stage of labour in which the bag of waters was broken. The eighth, ninth, and tenth columns show how many of the three membranes were tested simultaneously. The eleventh, twelfth, and thirteenth columns show what membranes gave way in each experiment. The fourteenth column states the radius of the circular mouth of the apparatus to which the membranes were tied. The fifteenth gives the barometric pressure at the time of each trial; and it will be observed that the pressure occasionally required a correction which demands explanation. The column of mercury in the apparatus was generally very short, and no correction for its weight was required, the experiments not pretending to an extreme nicety; but occasionally (in the cases noted in the column of remarks) the column of mercury was too long to be neglected, and a correction was made for its length. The sixteenth column gives the length of the column of air enclosed in the vertical tube above the mercury. The seventeenth gives the contraction of this column of air, by the pressure of water which burst the membranes, acting on the short column of mercury. The eighteenth column gives the height of the membrane as it bulged above the mouth of the apparatus, expanded by the water-pressure. The nineteenth gives the effective pressure of the water, at the moment of bursting of the membrane, in inches of mercury. The twentieth gives the diameter of the sphere, of which the membrane when bursting approximately formed a portion. The twenty-first column gives the pressure per square inch of the membrane at the time of the bursting of the membrane, or at the time of the experiment's failing from some cause, such as the slipping of the membrane. The twenty-second column gives the pressure on a circular surface of 2·25 inches radius, or equal to the assumed dimensions of the lumen of the passage through which the child is expelled. The twenty-third column gives the tensile strength of the membrane, or, in other words, the weight which a band of it, an inch broad, would bear without giving way.

TABLE OF EXPERIMENTS,

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.
							Membranes Tested.			Membranes Burst.			
No. of experiment.	No. of Case.	Length of Labour till rupture of Membranes.	Length of First Stage of Labour.	Length of Second Stage of Labour.	State of *Os uteri* at time of Rupture.	Stage in which Rupture occurred.	Amnion.	Chorion.	Decidua.	Amnion.	Chorion.	Decidua.	Radius of Apparatus.
		h. m.	*h. m.*	*h. m.*	*inch.*								*a*
1	1						×	×	×	×	×	×	1·675
2	,,						×	×	×	×	×	×	,,
3	,,						×	×		×	×		,,
4	,,						×			×			,,
5	2						×	×	×			×	,,
6	,,						×	×					,,
7	,,						×	×		×	×		,,
8	,,						×						,,
9	,,						×	×	×			×	,,
10	,,						×	×					,,
11	,,						×	×		×	×		,,
12	,,						×						,,
13	3						×	×	×	×	×	×	,,
14	,,						×	×	×			×	,,
15	,,						×	×					,,
16	,,						×						,,
17	,,						×						,,
18	,,						×						,,
19	4						×	×	×	×	×	×	,,
20	,,						×	×	×			×	,,
21	,,						×	×			×		,,
22	,,						×						,,
23	,,							×			×		,,
24	5	28 35	28 30	0 25		2d	×	×		×	×		,,
25	,,	,,	,,	,,		,,	×	×			×		,,
26	,,	,,	,,	,,		,,	×			×			,,

THE DEDUCTIONS THEREFROM.

16. Length of Column of Air.	17. Contraction of Column of Air.	18. Bulge of Membrane at time of Bursting.	19. Effective Pressure at Bursting, in inches of Mercury.	20. Diameter of Sphere of Membrane at Bursting.	21. Pressure on Square Inch.	22. Pressure on a Circular Surface of 2·25 inches radius.	23. Tensile strength of Membrane.	REMARKS.
l	λ	h	$\frac{b\lambda}{l-\lambda}$.	$h+\frac{a^2}{h}$.	p $\cdot 49\frac{b\lambda}{l-\lambda}$.	$1{\cdot}73\frac{b\lambda}{l-\lambda}\cdot\left(h+\frac{a^2}{h}\right)$.	T $\cdot 123\frac{b\lambda}{l-\lambda}\cdot\left(h+\frac{a^2}{h}\right)$.	
19·75	2·		3·32		1·63			
”	2·		3·32		1·63			
”	1·		1·57		·77			
”	3·25	1·5	·58	3·37	2·85	33·87	2·41	
”	·5		·766		·37			Membrane taken close to placenta.
”	1·5		2·42		1·19			Membrane slipped out.
”	·75		1·16		·57			
”	1·25		1·99		·97			Membrane slipped out.
”	1·		1·57		·77			
”	1·5		2·42		1·19			Membrane slipped out.
”	1·25		1·99		·97			
”	2·		3·32		1·63			Membrane slipped out.
20·	1·25		1·96		·96			Membrane taken close to placenta.
”	·75		1·15		·56			
”	1·2		1·88		·92			
”	·6		·912		·45			
”	·4		·602		·29			
”	·6		·912		·45			
”	1·1		1·72		·84			
”	·5		·756		·37			
”	·5		·756		·37			
”	·8		1·23		·60			A considerable leak in the membrane.
”	·2		·298		1·46			
16·5	2.5	·75	4·98	4·49	2·46	37·58	2·75	Barometer corrected by 1·6 inch.
”	2.	·75	3·78	4·49	1·85	29·36	2·09	Barometer corrected by 1·6 inch.
”	2·25	1·	4·40	3·80	2·16	28·96	2·06	Barometer corrected by 1·6 inch

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	1
27	5	28 35	28 30	0 25		2d	×	×		×	×		1·675	29·
28	"	"	"	"		"	×			×			"	"
29	6	6 0	4 55	1 10		2d	×	×			×		"	"
30	"	"	"	"		"	×			×			"	"
31	"	"	"	"		"	×			×			"	"
32	7	6 0				2d		×	×		×	×	"	"
33	"	"				"		×	×		×	×	"	"
34	8	5 20	6 25	3 15	1½	1st		×	×		×	×	"	"
35	"	"	"	"	"	"	×			×			"	"
36	"	"	"	"	"	"	×			×			"	"
37	9	7 10	7 0	1 30		2d	×			×			"	"
38	10	6 15	6 5	0 20		2d	×	×	×	×	×	×	"	"
39	11	6 15	4 10	7 15		2d		×	×		×	×	"	29·
40	"	"	"	"		"		×	×		×	×	"	"
41	"	"	"	"		"		×	×		×	×	"	"
42	12	21 0	24 0	A few minutes	2½	1st	×			×			"	29·
43	"	"	"	"	"	"	×			×			"	"
44	"	"	"	"	"	"	×			×			"	"
45	13	13 0	12 45	0 45		2d		×	×			×	"	"
46	"	"	"	"		"		×			×		"	"
47	14	11 0	10 15	0 30?		2d	×			×			"	"
48	"	"	"	"		"	×						"	"
49	"	"	"	"		"	×			×			"	"
50	"	"	"	"		"	×			×			"	"
51	"	"	"	"		"	×						"	"
52	"	"	"	"		"	×						"	"
53	15			7 30		1st	×	×					"	28·
54	"			"		"	×	×			×		"	"
55	"			"		"	×						"	"
56	"			"		"	×			×			"	"
57	16	6 30				2d	×			×			"	29·
58	"	"				"	×			×			"	"
59	17	3 30	9 30	1 0	½	1st	×			×			"	"
60	"	"	"	"	"	"		×	×		×	×	"	"
61	18						×						"	"
62	"						×			×			"	"
63	"						×			×			"	"
64	19		6 0	1 0		2d	×			×			"	29·
65	"		"	"		"	×			×			"	"
66	"		"	"		"	×			×			1·125	"
67	"		"	"		"	×			×			"	"

16.	17.	18.	19.	20.	21.	22.	23.	
16.5	2·25	·9	4·40	4·02	2·16	30·76	2·17	Barometer corrected by 1·6 inch. A leak in the membrane.
16·25	2·5	1·	5·07	3·80	2·48	33·37	2·37	Barometer corrected by 1·6 inch.
„	1·5	·75	2·83	4·49	1·39	15·06	1·07	Barometer corrected by 1·6 inch.
„	1·	·75	1·83	4·49	·90	14·21	1·01	Barometer corrected by 1·6 inch.
16·5	2·25	·75	4·40	4·49	2·16	31·18	2·43	Barometer corrected by 1·6 inch.
16·4	·6	·75	1·06	4·49	·52	8·23	·58	Barometer corrected by 1·6 inch. Birth 30 min. after rupture.
16·3	·5	1·	·882	3·80	·43	5·81	·41	Barometer corrected by 1·6 inch.
16·5	·4	·75	·693	4·49	·34	5·38	·38	Barometer corrected by 1·6 inch.
„	·9	1·	1·61	3·80	·79	10·60	·75	Barometer corrected by 1·6 inch.
„	·9	1·25	1·61	3·49	·79	9·73	·69	Barometer corrected by 1·6 inch.
16·2	·6	1·	1·07	3·80	·52	7·04	·50	Barometer corrected by 1·6 inch.
„	·3	·75	·526	4·49	·26	4·08	·29	Barometer corrected by 1·6 inch.
20·	·75	·75	1·15	4·49	·56	8·93	·63	
„	·75	·75	1·15	4·49	·56	8·93	·63	
„	·4	·25	·604	11·47	·30	11·99	·85	
20·5	1·8	1·	2·83	3·80	1·39	18·63	1·32	
„	·9	·75	1·35	4·49	·66	10·48	·74	
„	1·3	·8	1·99	4·31	·97	14·83	1·05	
„	·5	1·	·736	3·80	·36	4·84	·35	
„	·45		·661		·32			
„	·7	1·	1·04	3·80	·56	6·85	·49	
„	·8	1·5	1·19	3·37	·58	6·94	·49	Membrane slipped out.
„	2·	2·	3·18	3·40	1·56	18·72	1·33	
20·75	1·5	1·5	3·29	3·37	1·12	13·35	·95	
20·5	1·5	1·5	2·32	3·37	1·14	13·52	·96	Membrane slipped out.
„	2·	2·	3·18	3·40	1·56	18·72	1·33	Membrane slipped out.
21·	1·5	1·	2·21	3·80	1·08	14·55	1·03	Membrane found to have been injured.
„	1·25	1·	1·82	3·80	·89	11·98	·85	
„	2·25	2·	3·45	3·40	1·69	20·31	1·44	
„	1·25	1·	1·82	3·80	·89	11·98	·85	
„	2·7	1·1	4·28	3·65	2·10	27·03	1·92	Child born 30 min. after rupture.
„	2·5	1·	3·92	3·80	1·92	25·80	1·83	
21·2	·5	·5	·700	6·11	·34	7·40	·53	
„	·3	·5	·416	6·11	2·04	4·40	·31	
20·4	1·1	·8	1·65	4·31	·81	12·29	·87	Membrane slipped out.
„	1·25	1·	1·89	3·80	·93	12·44	·88	
20·5	2·3	1·5	3·66	3·37	1·79	21·34	1·52	
19·4	1·5	1·	2·45	3·80	1·20	16·13	1·15	
„	1·6	1·5	2·62	3·37	1·28	15·27	1·09	
22·3	2·	·75	2·87	2·44	1·41	12·10	·86	
„	1·7	·5	2·41	3·03	1·18	12·64	·90	An ill-conducted experiment.

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.
68	19	3 30	6 0	1 0		2d	×						1·125
69	"		"	"		"	×			×			"
70	"		"	"		"	×			×			"
71	"		" "	"		"	×			×			"
72	"		"	"		"		×	×		×	×	"
73	20	9 15	8 20	0 55		3d		×	×		×	×	"
74	21		9 0	3 0		"	×			×			"
75	"		"	"		"	×			×			"
76	22	1 30	2 0	1 0		2d	×			×			"
77	"	"	"	"		"	×			×			"
78	"	"	"	"		"	×			×			"
79	"	"	"	"		"	×			×			"
80	23		Long	1 0			×			×			"
81	"		"	"			×			×			"
82	"		"	"			×			×			"
83	"		"	"			×			×			1·675
84	24	32 30	30 0	4 0		2d	×			×			1·125
85	"	"	"	"		"	×			×			"
86	"	"	"	"		"	×			×			"
87	"	"	"	"		"	×			×			"
88	"	"	"	"		"	×			×			1·675
89	25						×	×		×	×		1·125
90	"						×	×		×			"
91	"							×			×		"
92	"						×			×			"
93	"						×			×			1·675
94	"						×			×			"
95	"						×			×			"
96	"						×						"
97	"						×			×			"
98	26	10 10	8 30	2 0		2d	×			×			1·125
99	"	"	"	"		"	×			×			"
100	"	"	"	"		"	×			×			"

16.	17.	18.	19.	20.	21.	22.	23.	
22·3	2·3	1·1	3·36	2·25	1·65	13·08	·93	
„	1·9	·75	2·72	2·44	1·33	15·68	1·11	
„	2·5	·75	3·69	2·44	1·81	15·56	1·11	Burst by air contained between the membrane and water.
„	1·7	·55	2·41	2·85	1·18	11·88	·84	Burst by air contained between the membrane and water.
„	·9	·5	1·23	3·03	·60	6·45	·46	
„	1·2	·6	1·66	2·71	·81	7·78	·51	Membranes not ruptured till after birth of head.
22·2	1·2	·6	1·67	2·71	·82	7·83	·56	Membranes ruptured with first pain of labour.
„	1·7	·8	2·42	2·38	1·19	9·97	·71	
22·	2·	·5	2·98	3·03	1·46	15·62	1·11	A large leak in the membrane.
„	2·4	·65	3·65	2·60	1·79	16·40	1·17	A small leak in the membrane.
„	3·2	·75	5·07	2·44	2·48	21·37	1·52	
„	2·85	·6	4·43	2·71	2·17	20·76	1·48	
„	2·3	·7	3·48	2·51	1·70	15·10	1·07	
20·	1·8	·6	2·91	2·71	1·43	13·64	·97	
„	2·1	·5	3·49	3·03	1·71	18·30	1·31	A small leak in the membrane.
19·3	1·5	1·05	2·51	3·72	1·23	16·16	1·15	
19·8	1·	·6	1·58	2·71	·77	7·40	·53	Membranes ruptured in labour by attendant.
19·5	1·4	·7	2·30	2·51	1·13	9·98	·71	Membranes ruptured in labour by attendant.
„	1·7	·75	2·84	2·44	1·39	11·97	·85	Membranes ruptured in labour by attendant.
„	·9	·55	1·44	2·85	·71	7·10	·50	Membranes ruptured in labour by attendant.
19·3	1.4	·85	2·33	4·15	1·14	16·73	1·19	Membranes ruptured in labour by attendant.
22·	3·4	·65	5·36	2·60	2·63	24·08	1·71	Barometer corrected by ·5 inch.
„	3·6	·8	5·74	2·38	2·81	23·65	1·68	Barometer corrected by ·5 inch.
„	1·8	·9	2·61	2·31	1·28	10·41	·74	Barometer corrected by ·5 inch.
„	3·9	·75	6·32	2·44	3·10	26·64	1·89	Barometer corrected by ·5 inch.
19·2	1·25	·8	2·08	4·31	1·02	15·50	1·10	A leak in the membrane.
19·	1·3	·8	2·19	4·31	1·07	16·32	1·16	
„	1·45	·85	2·46	4·15	1·20	17·66	1·26	
„	2·	1·5	3·51	3·37	1·72	20·46	1·45	Membrane slipped out.
„	1·7	1·	2·93	3·80	1·44	19·29	1·37	Same piece as used in last experiment.
22·	1·5	·6	2·15	2·71	1·05	10·07	·72	Barometer corrected by ·5 inch.
„	1·6	·7	2·30	2·51	1·13	9·98	·71	Barometer corrected by ·5 inch.
„	1·8	·6	2·61	2·71	1·28	12·23	·87	Barometer corrected by ·5 inch.

Professor Tait has supplied the following formulæ from which the columns of the tables are computed :—

Let b be the height of the barometer, corrected for the short column of mercury in the gauge;

l the length of the air-column before pressure is applied;

λ the contraction of the column when the membrane bursts.

Then, since the weight of a cubic inch of mercury, at ordinary temperatures, is about 0·49 lbs., we have, for the difference of pressures on opposite sides of the membrane when it bursts, the expression

$$p = 0{\cdot}49\, b \left(\frac{l}{l-\lambda} - 1\right) = 0{\cdot}49\, \frac{b\lambda}{l-\lambda} \quad . \quad (1).$$

in pounds per square inch. No sensible correction is required for the length of the water-column, when the mercury in the gauge and the membrane were not exactly at the same level.

If T be the force in pounds weight which will just snap a band of the membrane an inch broad, ϱ the radius of curvature when the membrane bursts, we have, by a known theorem, the membrane being supposed to form approximately a portion of a sphere,

$$\frac{2T}{\varrho} = p \quad . \quad . \quad . \quad . \quad (2).$$

To find ϱ, we remark that the external semidiameter of the apparatus a is the radius of the base of a spherical segment, whose height h is measured; and geometry gives at once the equation

$$2\varrho = h + \frac{a^2}{h} \quad . \quad . \quad . \quad (3).$$

Hence, the tensile strength of the membrane is

$$\mathrm{T} = 0{\cdot}123\,\frac{b\lambda}{l-\lambda}\left(h + \frac{a^2}{h}\right) \quad . \quad . \quad (4).$$

If we assume that the membrane is usually burst, by natural processes, when a portion of it forms a hemisphere of 2·25 inches radius, the requisite pressure in pounds per square inch will be, by (2) and (4)

$$\frac{0{\cdot}245}{2{\cdot}25}\,\frac{b\lambda}{l-\lambda}\left(h + \frac{a^2}{h}\right) \quad . \quad . \quad (5).$$

and the effective pressure, on a circular surface of 2·25 inches radius, will then be

$$\pi\,(2{\cdot}25)^2\,\frac{0{\cdot}245}{2{\cdot}25}\,\frac{b\lambda}{l-\lambda}\left(h + \frac{a^2}{h}\right) = 1{\cdot}73\,\frac{b\lambda}{l-\lambda}\left(h + \frac{a^2}{h}\right) \quad (6).$$

In making such experiments, a small given error in the estimate of the depth of the approximately spherical segment will be of least consequence when the membrane bursts in a nearly hemispherical form, for by (3)

$$2\delta\varrho = \delta h\left(1 - \frac{a^2}{h^2}\right)$$

and the error in the estimated radius vanishes, if $h = a$. Hence, also, the assumption that, in nature, the rupture takes place when the protruded portion of the membrane is hemispherical, gives a *minimum* value of the whole extruding force.

For the purposes of this paper the greatest value of the Table lies in the twenty-second column, which gives the power of the labour at the time of the rupture of the membranes and evacuation of the liquor amnii, on the supposition that the lumen of the passage opened up was circular, and of 4½ inches in diameter, and that the bulge was hemispherical at bursting. The first striking observation to be made, is the great variation in the strength of the bag of membranes. The force required to rupture the weakest amnion showed that the power of the labour was at least 4·08 lbs.; that for the strongest, a power of 37·58 lbs.; and the average power indicated by the experiments on the amnion was 16·73 lbs. The average tensile strength was 1·19 lbs. Next, it is to be remarked, that in the cases whose membranes were tried, the power of labour almost certainly exceeded the power required to burst the bag, for it is not probable that a particularly weak small portion, unlike the rest of the membrane, was ruptured in the labours.

In cases 5, 6, 10, 14, 16, 26, the labour did not last above half-an-hour after the rupture of the membranes; and the greatest power indicated experimentally by rupturing the membranes was in each case respectively 37·58 lbs., 31·18, 4·08, 18·72, 27·03, 12·23.

In case 22, it was particularly observed that, so far as I could judge, the pain rupturing the bag was stronger than any that followed; it may therefore be supposed, that the power terminating labour little exceeded 21·37 lbs., the greatest power indicated by the experiments as rupturing the membranes of that patient's ovum.

The same contractile force of the uterus at different periods of labour, or, to be more exact, at different dimensions of the uterus, will produce greater internal pressure, and consequently greater expulsive force, as the uterus is smaller (*vide* equation (2), p. 312); and, the amount of muscular contraction being supposed to be the same, there may be no sign to the attendant or patient of the increase of power. This may be restated in a manner more pertinent to the actual facts. It is a common belief that uterine pains increase in strength after the evacuation of the liquor amnii. Whether this be true or not, as commonly believed, I do not here consider. But it is certain that, if the uterine contractions remain of the same force after, as they had before, the partial evacuation of the liquor amnii, the power of the labour, or the extruding force, will be increased, as curvature of the contracting organ is increased. On the other hand, the application of the same principle shows that when the curvature of the extruded portion of the membranes is greatest, the difficulty of rupturing them is also greatest. This occurs when the extruded portion is hemispherical; and it is on this supposition that the numbers in column 22 are calculated.

It was only after conceiving the means above described for arriving at the conclusions of this paper, and after the plan of the apparatus had been made by Professor Tait, that I fell in with an interesting and valuable paper by Dr. J. Poppel of Munich—"Ueber die Resistenz der Eihäute, ein Beitrag zur Mechanik der Geburt," contained in the first part of the twenty-second volume of the *Monatsschrift für Geburtskunde* for 1863. This paper anticipates to a very great degree the plans and results here related. But it may be pointed out that Dr. Poppel has neglected to note some conditions of the experiment, which cannot be omitted without damaging materially the accuracy and value of the trials; especially he has always supposed the membrane to burst, in the experiments, when in a hemispherical form, which is certainly an error, and one whose tendency is always to make the strength of the membrane too little [*vide* equation (2)]. He has attached some weight to the part of the amnion tested, considering that greater strength would accompany proximity to the placenta; but my experiments did not confirm this opinion.

Dr. Poppel's apparatus may be sufficiently, though not fully, described as follows:—The membrane to be tried he ingeniously fixed over one or other of two glass vessels, of the diameter of five centimetres or two inches, and of ten centimetres or four inches, respectively. The glass vessels were reagent glasses, from which the bottoms were taken off. The affixed membranes represented the bottoms of the reagent glasses. Into the corks of the glasses a long glass tube was passed. Through this tube mercury was poured into the bottle till it filled it, and mounted into the tube. Its height in the tube at the time of the bursting of the membranes was carefully noted, because from it was estimated the pressure that burst the membrane. In adding the mercury fitfully, Dr. Poppel erroneously supposed that he imitated the pains of labour, a point, it appears to me, of no importance; and besides, his idea was manifestly erroneous, for each succeeding pain is not an addition to a force previously in action—it may even be weaker than its predecessor. In every natural case it is an entirely new force, rising in strength from zero to its acme, and again gradually fading to zero. Dr. Poppel made allowance for the weight of mercury contained in the reagent glass, over and above what was in the vertical glass tube; but he neglected the important element of the degree of bulging of the membrane, or radius of its curvature at time of bursting, with a view to arriving at the diameter of the globe, of which it formed a section at the time of rupture. With this he connects also a statement, that the bulging of the membranes through the mouth of the womb rarely exceeds a hemispherical form, which, though perhaps nearly true, is misleading, if held to be true in regard to the class of cases of persistent membranes specially studied in this paper.

The average strength of the amnion found by Poppel was, keeping an aperture of 2·25 inches in radius in view, 19·21 lbs.; in my experiments it was 16·73 lbs.

Poppel experimented on the membranes in seven cases in which they burst "with the birth." The a diameter of 4 inches, and as I prefer regarding it as nearer 4½, so I, using meantime Poppel's experiments and calculations, make the power exerted in an easy labour vary from about 6 lbs. to about 27 lbs., instead of from 4 to 19. I shall not meantime attempt to show whether Poppel's assumed 4-inch diameter or my assumed 4½-inch diameter is the more likely to be nearest the truth, because it would lead me into a class of questions remote from the subject-matter of this paper.

If we observe that in Poppel's table of experiments and in mine the power shown to be sufficient to terminate an easy labour was often far exceeded in the course of other labours, we may enunciate the almost certain conclusion that a great mass of easy, and not merely of the easiest, labours is terminated by a power little in excess of that required to rupture the bag of membranes. The strongest membrane found in the experiments indicated, by the pressure required to burst it, an extruding force of 37½ lbs. We may therefore, I think, safely venture to assert, as a highly probable conclusion, that the great majority of labours are completed by a propelling force not exceeding 40 lbs.

If we regard the figure of 4 lbs. given by Poppel as equal to the power exerted in the easiest labour he has observed, or the corresponding figure of 6 lbs. according to my calculations, and keep in mind that the average weight of the adult fœtus exceeds either of these weights, we are led to the conclusion that in the easiest labours almost no resistance is encountered by the child; that it glides into the world propelled by the smallest force capable of doing so; that, with the mother in a favourable position, the weight of the child is enough to bring it into the world—a result which many clinical facts at least appear to confirm.

Having thus given Poppel's and my own estimate of the force exerted in natural parturition of the easiest kind, I can at present offer nothing positive from which to calculate the strength of labour in the general run of cases. My belief is, that in ordinary labours the power exerted is not in general much above the lower limit; but other accoucheurs may see reason to entertain different opinions.

CHAPTER II.

THE GREATEST POWER OF LABOUR EXERTED IN DIFFICULT CASES.

THE higher limit of the power exerted in natural parturition has been variously estimated. There is an easy and obvious method of arriving at it. Cases are frequently occurring in which labour is artificially terminated by forceps, in circumstances which leave no doubt that, under delay, they would have come with difficulty to a spontaneous conclusion. The power exerted by the forceps in such cases can be measured. Such measurements are not to be at once taken as the power of labour necessary to finish such cases; but when all of the various sources of error are considered and included, they are of much value. The chief of such sources of error are the neglect of the assistance that may be afforded to the operator by the natural expulsive efforts, and the including of such forces, exerted by the forceps, as may be unnecessary for carrying on the process; for example, prematurely applied force, or force applied so as to advance the birth too hastily, or force lost by being used in a wrong direction. For the making of observations of this kind by the forceps, special instruments have been inven by Kristeller, Joulin, and others.

But forceps-cases do not afford the only evide available as to the higher limit. Experiments can made on the dead subject which can be very relied upon, as reproducing correctly the difficu encountered in the living, and the power required overcome it. Such experiments have been made Joulin,* and when suitably arranged, give us power exerted in cases which the most powe parturient efforts might bring to a spontaneous termi tion; and, it may be added, would involve the moth life in great risk.

Speaking of these experiments, Joulin makes following remarks:†—"Spontaneous delivery has b sometimes observed in circumstances almost identi It appears to me, therefore, possible to admit that figure of 50 kilogrammes (about a hundredweight force represents very nearly the maximum of the c tractile power of the uterus; for it is necessary take into account the accessory contingent furnis by the abdominal muscles, which in these instan was awanting. But as this force has not a di action, it is probable that its actual product scarc rises above a few kilogrammes." On this quotat from Joulin I shall make two remarks, first, that labour the accessory contingent furnished by the abdominal muscles, appears to me to have an action nearly, if not quite, as direct as that of the uterus itself; and second, that I know of no means whatever of estimating its value. In my experiments and descriptions I treat of the powers of labour, that is, of all the powers exerted in combination. Yet there can be no doubt that the paramount power is that produced by uterine contractions.

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* *Mémoires de l'Académie Impériale de Médecine.* Tome xxvi 90, etc. See also his *Mémoire sur l'Emploi de la Force en Obstétr Archives générales de Médecine: numéros Février et Mars* 1867.

† *Traité Complet des Accouchements*, p. 447.

Having had extensive and varied experience in the use of the forceps in difficult labours, and having also made some rough experiments with the dynamometer, to ascertain the power I have applied by the instrument, I regard M. Joulin's estimate of a hundredweight as the maximum force of the parturient function, as too high. I do not deny that, in very rare cases, such a force may possibly be produced; but I am sure that it is nearer the truth to estimate the maximum expulsive power of labour (including the uterine contractions with the assistant expulsive efforts) as not exceeding 80 lbs.

At present, I can divine no method of arriving at an estimate of the expulsive power of labours generally except the following; and I must guard myself from being supposed to recommend its use, in the meantime at least. A fine tube, filled with water and of resisting material, may be introduced into the small pool of liquor amnii which remains after the rupture of the membranes, filling up the spaces otherwise vacant on the anterior aspect of the fœtus. This tube should be provided with an aperture at its uterine end; it should be curved, so that when introduced it may lie easily in the pelvis, occupying the least possible space, so that no unnecessary resistance be offered to the advance of the fœtus; its wall should taper to either side, a cross section of it having a long pointed fusiform outline, in order that its presence may not produce on either side of it a channel for the running off of the pool of liquor amnii; lastly, its external end should be in communication with a column of mercury in a vertical tube, enclosing a column of air under only ordinary barometrical pressure. During the pains the rise of the mercury in the tube may be measured, and calculations from these measurements might be made, identical with those already given in the former chapter of this part. By this means, if successfully applied, the force of any labour may be exactly known. And it is scarcely necessary even to suggest how immeasurably valuable to the accoucheur such an estimate would be, substituting, as it would, an experimentally accurate statement of great importance for the vague notions at present relied on, even when the wisest and most experienced practitioner lends his counsel.

There can be no doubt as to the great practical importance of the inquiry entered upon in these chapters. Although it is, as yet, far from completed, there is enough demonstrated to enable Dr. Slop, if he have an opportunity, to cast ridicule on the father of Tristram Shandy, who, founding on the statements of

Lithopœdus Senonensis, asserts that the force of a woman's efforts is, in strong labour-pains, equal, upon an average, to the weight of 470 lbs. avoirdupois, acting perpendicularly upon the head of the child!!

It would be interesting to know the source, if any, whence the Rev. Laurence Sterne drew his information as to the power of labour in days when little attention was paid to this subject. But it is more important to look forward and anticipate the advantages which a handy available dynamometer of parturition would afford to the practitioner. They spring up to the thoughtful mind so readily as scarcely to call for statement,—when complaints are severe, or cries for relief urgent, to have a means of judging what is really the power of the labour; when a case is protracted, to have a means of estimating the pressure exerted on the child and maternal passages; when, in any case, instrumental interference is under consideration, to have a means of judging if the error is in the force exerted or in the resistance, etc. etc. That some means of thus increasing the beneficent power of the accoucheur will be discovered, I confidently anticipate.

CHAPTER III.

ON THE POWER OF THE UTERUS TO RESIST A BURSTING PRESSURE.

RUPTURE of the uterus, the accident in nature which the following experiments most nearly imitate, is so interesting and important, and as yet so imperfectly studied, that anything tending to throw light upon it is valuable.

Before recording the experiments, it is necessary to state that they were conducted with nearly the same machinery, and on the same principles, as those already described as made upon the bag of membranes. As the power used was found to be great, the apparatus was sometimes immersed in water, to avoid the disagreeable little accidents that might have arisen to the bystanders from the bursting in air. In the experiments made with air, it was curious to observe the permeability of all the unruptured tissues to this fluid. This, of course, did not vitiate the experiments, but it was unexpected to observe its effervescing, as it were, through the peritoneum covering the part experimented on.

The great, the enormous strength of the uterus also attracts particular attention. Most of the experiments, in one sense—indeed, all—failed from deficiency of power in the apparatus used. And, though Professor Tait is now provided with an apparatus of power enough, I have unfortunately been unable to procure material for further experiments. The uterus used in our trials was kindly sent me by Dr. Miller of Dundee. It was entire, and contained an entire fully developed ovum. When I got it, and during most of the experiments, it was fresh, but in some of them it was found that parts had begun to decompose. Its structure was microscopically examined by Professor Turner, who found it to be healthy, some of the fibres only of the muscular tissue having undergone fatty degeneration, and that only very partially, even in the individual fibres showing fat-granules distinctly.

Although the experiments are very defective, they yet afford some important results. One of them is the immense superiority of the power of the uterus to resist a bursting pressure, to that believed to be exerted by it in its moments of greatest effort. In other words, the experiments show that Nature has provided in the healthy uterus an apparatus possessed of far greater capabilities, in a mechanical point of view, than are ever called into exercise. Were the uterus destined to do far harder work than it ever attempts, it needs no increase of strength of materials. From this it follows, as a necessary conclusion, that it never is spontaneously ruptured when it is healthy. For the explanation of the occurrence of spontaneous rupture, it is necessary to invoke the softening and weakening influence of inflammation, or some thinning or other morbid change.

I am indebted to Dr. G. W. Balfour for pointing out to me the opinion of Casper in regard to the increase of strength in dead structures, and that this condition of dead tissues may vitiate my experiments. I shall give this opinion of Casper in the words of Dr. Balfour's translation:—"In § 33, and its illustrative cases, I have shown how often, in cases of sudden death from injuries—when death has occurred from internal and not from external causes—there is often no trace of injury visible externally which could betray the nature of the case. This circumstance, as well as the desire to investigate how far it was possible for a criminal to mask the actual cause of death, and obscure the case by producing injuries on the body of the deceased, just as murderers often attempt to conceal their deed by burning the body; as well as to discover what relation the resistance of the dead organ bore to that of the living one, led to the experimental production of injuries upon dead bodies. I have had uncommonly numerous opportunities of instituting these experiments, and I still continue to repeat them every academical session. Similar experiments have not previously been made anywhere on so great a scale —except in the case of experiments on burning, to which I shall refer under the head of 'Death from Burning'—and they have been attended by the most astonishing results. It is extremely difficult to break up the organic cohesion of dead organs. In saying this, of course I do not refer to stabs or cuts through the skin and muscles. Our experiments, in respect to mechanical injuries, were confined to fractures of the bones, ruptures of the internal organs, and injuries (alterations) of the cuticular surface." After giving his experiments on fractures of the bones, he continues: "We have only made a few experiments upon the rupture of internal organs, because no important practical result was to be expected from them. The most violent blows, with beams and the like, upon the regions of the liver and spleen have not been attended with the slightest result."*

It is to be remarked, that even admitting to the full the force of Casper's numerous experiments, we are left with only Casper's individual opinion. For we have no statement, nor any good grounds for forming a reliable opinion, as to the force required to produce injuries on the living. And I must here take the liberty of simply stating that Casper has not convinced me of the truth of his opinion. I hold, indeed, in the meantime, that it is far from being proved. That dead tissues should have more strength than living ones is at least highly improbable, after all that Casper has said. I have indeed supposed that to some tissues vitality might impart force. I do not know if any experiments have been devised to show the comparative strength of muscles in action and muscles at rest, but I do regard it as a subject worthy of investigation.

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* *A Handbook of the Practice of Forensic Medicine.* Sydenham Society's Translation, vol. i. p. 45.

It seems possible that the contraction of a muscle might increase its cohesion, might augment for the time its tensile strength.

The last point in connection with the following experiments to which I shall call attention is the rupture, crack-like with irregular edges, of the peritoneum. The records of the details of the experiments show that the membrane repeatedly gave way, while the muscular part of the wall remained entire. This shows that it possesses a less degree of extensibility than the muscular layer, and may perhaps throw some light on those interesting and not extremely rare cases of crack-like fissuring or rupture of the peritoneum observed in some autopsies after parturition. It forms also a curious contrast, pressure and rapid distension easily rupturing a structure which is capable of growing or distending slowly to any extent, as is observed in pregnancy and ovarian dropsy.

TABLE OF EXPERIMENTS,
WITH THE DEDUCTIONS THEREFROM.

TABLE OF EXPERIMENTS, WITH

No. of Experiment.	Radius of Apparatus.	Barometric Pressure.	Length of Column of Air.	Contraction of Column of Air.	Greatest Bulge of Uterine Wall.	Greatest Effective Pressure, in Inches of Mercury.	Diameter of Sphere of Uterine Wall at Greatest Distension.	Pressure on Square Inch.
	a	b	l	λ	h	$\frac{b\lambda}{l-\lambda}$	$h+\frac{a^2}{h}$	$\cdot 49\frac{b\lambda}{l-\lambda}$
1	1·125	29·3	20·	7·	...	15·78	...	7·73
2	1·125	29·3	14·	8·	...	39·06	...	19·14
3	1·125	29·3	14·	6·5	...	25·39	...	12·44
4	1·125	29·3	12·	6·5	·75	34·63	2·44	16·97
5	1·675	29·3	20·5	12·5	·75	45·78	4·49	22·43
6	1·85	29·3	10·75	5·75	1·25	33·69	3·91	16·51
7	1·85	29·3	9·25	5·	8·	34·47	4·95	16·89
8	1·85	29·3	8·25	5·25	1·	51·27	4·32	25·12
9	1·25	29·3	8·	4·5	·75	37·67	2·83	18·46
10	1·25	29·3	8·	4·	·75	29·30	2·83	14·36

THE DEDUCTIONS THEREFROM.

Tensile Strength of Uterine Wall.	REMARKS.
$\cdot 123\frac{b\lambda}{l-\lambda}\cdot\left(h+\frac{a^2}{h}\right)$	
...	Air, forced by a pump, was used in this experiment and in the two following. In all three it came freely through the uterus by innumerable apertures. The part tested was taken from the upper part of the posterior wall of the uterus. The placenta was attached to the anterior wall. The part experimented on was not burst. It slipped off the apparatus.
...	The same part used as in last experiment. Like the last, the experiment was imperfect. The uterus was not burst. The peritoneum gave way, forming a long fissure.
...	The same part used as in last experiment. The india-rubber pipe of the apparatus gave way.
10·39	Water-pressure was used in this and the succeeding experiments. The part experimented on slipped.
25·28	The part experimented on slipped. It was taken from anterior wall, low down.
16·20	In this experiment only the internal half of the texture slipped. The uterus was thus not burst through its whole thickness.
20·99	The part is from anterior wall, not placental. The part burst was thin and softened by decomposition.
27·24	The part used was placental. The peritoneum alone gave way. It presented numerous elongated, irregular, cracks.
13·11	The part used was placental. Peritoneum cracked and partly peeled off.
10·20	The part used was the same as last. Numerous unsuccessful efforts were subsequently made to burst this by using the pump, but they all failed. The india-rubber junction-pipe gave way.

CHAPTER IV.

OBLIQUITY OR LATERAL FLEXION OF THE FOETAL HEAD IN THE MECHANISM OF PARTURITION.

THE object of this paper is to show that the obliquity, or lateral obliquity, of the fœtal head when passing through the brim of the pelvis, described by Næegele, by some of his predecessors, and by his followers down to the latest authors, does not exist in natural parturition; and that obliquity, or lateral obliquity, of the fœtal head when passing through the outlet of the pelvis, not described as such by Næegele and most of his followers, does occur in natural parturition.

This word, obliquity, and the alternative expression, lateral obliquity, are terms requiring, for most readers, some explanation; their meaning certainly not lying on the surface. The words ordinarily used to express the state, considered as a fœtal attitude, seem to me far more appropriate, and scarcely susceptible of being misunderstood—viz. lateral flexion, a term which Stoltz has applied to it.

I have for many years ceased to teach in my class-room the doctrine of Næegele on this subject. At the same time, I need scarcely assure my professional brethren that it is with the greatest diffidence that I venture to adduce views opposed to those of Nægele, of Dubois, and of subsequent authors too numerous to mention. It is for the profession to decide whether truth lies with them or with me.

But I am not alone and unsupported in the views I entertain. Many authors, indeed, enter at length into descriptions of, and arguments for, Nægele's opinions; but I know of none who has taken up the opposite side with any fulness. At the same time, I have, since writing my first paper on this subject, found, on reference to the works of Velpeau,* Cazeaux,† and of Dr. R. U. West,‡ that these gentlemen have reached conclusions similar to my own regarding the direct entrance of the fœtal head into the pelvis.

Since my views on this subject were first published in 1861, there has appeared a treatise on the mechanism of parturition by Dr. W. Leishman, which contains what appears to me to be far the best exposure of the errors of Nægele, and the best defence of the views which I entertain. Dr. Leishman has almost justly characterised my statement as wrong, when I said in the place referred to "that obliquity, or lateral obliquity, of the fœtal head when passing through the outlet of the pelvis, not described by Nægele and his followers, does occur in natural parturition." To make my statement quite correct, I should have inserted the words "as such" into the above quotation, making it “not described as such by Nægele and his followers.” With this correction I reassert my former statement. I knew, indeed, that Nægele's essay cannot be understood without supposing he believed there was lateral flexion at the outlet, in a direction opposite to what is supposed by him to occur at the brim; and Dr. Leishman, even, admits that in the part of Nægele's essay referring to this point he is unfortunately a little vague in his details—a fault, indeed, which is scarcely to be found elsewhere in his admirable essay.* It appears to me that I had been, so far, misled by desire to state the point antithetically.

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* *Traité Complet de l'Art des Acc.* Bruxelles, 1835, p. 250.

† *Traité de l'Art des Accouch.* 1858, p. 423.

‡ *Glasgow Medical Journal*, 1857, p. 304.

Hubert seems to hold an opinion midway between Nægele's and that here supported. “The head,” he says,† “is slightly inclined in such a manner that the anterior parietal bone is found a little lower than the other, not only as regards the horizon, but also as regards the plane of the superior strait of the bony pelvis.” . . . “The two opinions (Nægele's and Cazeaux's) appear to us both admissible in given cases; a head of little bulk, a large pelvis, an inferior segment of the uterus well hollowed out, a uterus well placed in the axis of the upper strait, permit the vault of the cranium to present itself perpendicularly to the entrance of the pelvic canal; whilst a pelvis not very spacious, a narrow resisting inferior segment of the uterus, a bulky head, a prominent belly, will force the cranium to become inclined, and to offer to the plane of the upper strait the subparieto-superparietal diameter, instead of the biparietal properly so called."

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* *An Essay, Historical and Critical, on the Mechanism of Parturition*, p. 81.

† *Mém. de l'Acad. Roy. de Méd. de Belgique*, 1857, p. 118.

Nægele's teaching is to be found in his original paper, "Über den Mechanismus der Geburt," published in 1819 in *Meckel's Archiv für die Physiologie.* I shall quote from Dr. Rigby's translation of it, published in London ten years afterwards. Before doing so, I must premise that the quotations refer to the first or commonest position of the head in labour, and that in the whole of this paper I shall suppose this position to be understood. The doctrines apply to the other positions as well as to the first, and if proved or disproved for the one, stand or fall for the others.

"At the entrance of the pelvis (says Nægele), the head does not take a perpendicular, but a perfectly oblique direction, so that the part which lies lowest or deepest, is neither the vertex nor the sagittal suture, but the right parietal bone. The sagittal suture is much nearer to the promontorium of the sacrum than to the os pubis, and divides the os uteri, which projects backwards and generally somewhat to the left, across into two very unequal segments."—(P. 13.)

"The higher the head is . . . the more oblique is its direction; from which reason the right ear can generally be felt behind the pubes without difficulty, which would not be the case if the head had a perpendicular direction."—(P. 16.)

"On account of the oblique position of the head, the greatest width of the cranium (from one tuber parietale to the other), as well as that of its basis, can never, during its passage, coincide with the diameters of the pelvic entrance."—(P. 19.)*

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* The difference between Nægele's and my views is well shown, graphically, in the two woodcuts here given, and for which I am indebted to the kindness as well as to the pencil of Dr. Leishman, in whose work on the *Mechanism of Parturition* they have already appeared.

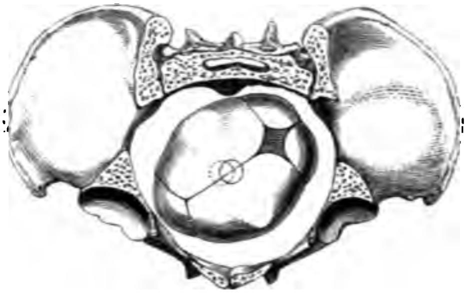

Fig. 34.

Fig. 34 is a representation of my description of this point in the mechanism. The head is descending directly. The reader is supposed to be looking from below upwards; and to avoid intercepting his view, the lower half of the bony pelvis has been sawed away.

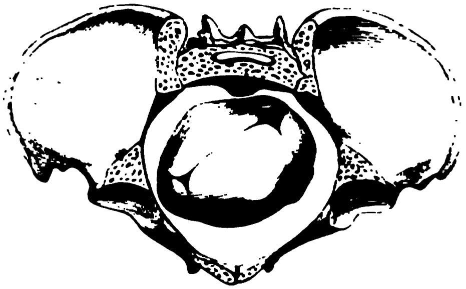

Fig. 35.

Fig. 35 shows Nægele's description of this point—that is, the head descending obliquely, or flexed laterally. In the general arrangements, the two cuts are identical.

In further illustration of this doctrine, I shall quote the account of it in Tyler Smith's *Manual,* a late British systematic work on midwifery:—

"The right side of the cranium is considerably lower than the left, so that the most depending part of the cranial surface is the protuberance of the right parietal bone. This lateral depression is called the obliquity of the head."—(P. 268.)

"The movement of the fœtal head upon its occipito-frontal axis. . . . This movement causes one side of the fœtal head to become lower than the other during the whole progress of labour, after the head has entered the brim, constituting the obliquity of the head."—(P. 274.)

If it be recollected that the axis of the child's body, the axis of the uterus, and the axis of the brim of the pelvis, are represented by the same line in the normal or standard condition, or very nearly so, then it is easy to see that this obliquity implies lateral flexion of the child's head, or the approximation of its left ear to its left shoulder. In respect of this lateral flexion, the child's attitude is thus represented as being changed from that maintained in utero before labour. The position of the child's head, as described in this changed attitude, is oblique; that is, the vertical axis of its head is said not to impinge upon the plane of the brim at right angles to it, but obliquely, forming an acute angle opening forwards and pointing backwards.

I do not believe that, in normal or standard conditions, any such lateral flexion or obliquity exists, but that Nægele and his followers are in error in this particular, and that the head enters the brim without any lateral flexion, and directly—that is, with its vertical axis at right angles to the plane of the brim.

I. The first and chief reason for denying the existence of obliquity of the fœtal head at the brim of the pelvis is, that it is not observed. I have been in the habit of carefully making out the position of the fœtal head in the brim of the pelvis at the beginning of labour; and although I have observed that it varies to a slight extent in different cases, I have satisfied myself that it enters the brim, very generally, directly and not obliquely.

The obliquity which is described has never been seen. It is only a supposed result of the observation of many particulars in many cases; and the observations are of a kind demanding great information and carefulness. The liability to be misled by preconceived theories is very great; and I cannot help thinking that the obliquity under discussion is the fruit of a mind powerfully impressed with the important part that obliquity undoubtedly plays in other departments of the mechanism of parturition.

I have not found the sagittal suture nearer the promontory of the sacrum than to the symphysis pubis. I have not found more of the right parietal bone approachable by the examining finger than would be so if the head entered the brim without obliquity. I have not found the parietal protuberance lying near the centre of the brim of the pelvis, nor approaching to it. I have not found the right ear of the child easily reached, while the vertex of the child was at or near the brim of the pelvis.

It is needless to pursue a career of assertions opposed to the statements of the believers in the obliquity. I shall proceed to evidence corroborative of the position I maintain. But before doing so, I shall simply mention that in cases in which, for various reasons, I have introduced my hand, and felt the whole head as it lies in the brim, I have not found the obliquity under discussion. This is valuable evidence, especially in cases where there is no deformity of the head or structures in the neighbourhood.

Leishman, in his work already referred to, has added another valuable evidence in favour of the belief that the sagittal suture of the head entering the brim of the pelvis is midway between the promontory and the pubes (p. 70). "For my part," says he, "I have left no means untried by which this might be tested. On introducing an instrument which is well known to surgeons as Professor Buchanan's rectangular staff for lithotomy, I have been able to place the angle on the second bone of the coccyx, inclining the short limb until it coincided, as nearly as I could guess, with the axis of the brim, when it never failed to guide me, if properly placed, to the sagittal suture, or some point very near it, on either side. I have even attempted a crucial experiment by measuring, by means of a flexible scale, the distance from the sagittal suture to the promontory of the sacrum on the one hand, and the pubes on the other; and although, for obvious reasons, the results were not so accurate as to warrant of themselves any definite conclusion, they certainly tended to confirm my belief."

Künekc has on this point the following remarks corroborative of my views,* whose intended force, however, in some subsidiary points, I do not admit. "Duncan (says he) investigates with the whole hand, and Leishman uses, for measuring, Professor Buchanan's rectangular staff for lithotomy. I hold both methods as at least superfluous, and am almost always satisfied with the method of examination which Michælis has taught us for measuring the diagonal conjugate—namely, by means of the simultaneously introduced middle and index finger. And here, if the fingers are only placed right in the axis of the pelvis, it becomes evident that the aspect of the sagittal suture as stated by Nægele is only apparent, and that in reality it stands at equal distance from the promontory and the symphysis."

II. The descriptions of the oblique position of the head given by Nægele and others are more truly applicable to its direct than to its supposed oblique position. This will be best shown by an examination of Nægele's remarks bearing on this matter.

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* *Monatsschrift für Geburtsk.* Bd. xxvii. 1866. S. 30.

"*The finger,*" says he, "*which is introduced in the direction of the central or middle line of the pelvic cavity, and brought in contact with the head, will touch the right parietal bone in the vicinity of its tuber.*" It is to be remarked, *firstly,* that the finger is not introduced in the central line of the pelvic cavity, but only in the direction of it; *secondly,* that it is not made evident that the head is to be touched or approached in the direction of the axis of that part of the pelvis where the head is lying—that is, in the axis of the brim. The finger may approach the head in the direction of the axis of the brim, but it is scarcely long enough to do so in the axis of the brim, or as nearly in it as, in the circumstances, can be guessed. The consideration of these points shows Nægele's statement to be so vague as to be without much value in this discussion. But it will be evident to all that the quotation cited is perfectly true, on the supposition that the head enters the brim directly, not obliquely.

To bear satisfactorily upon this question, Nægele's statement should have been to the effect, that the finger approaching the child's head in the axis of the brim, or where the imaginary axis of the brim passes through the surface of the vertex, touches the tuber of the parietal bone or its vicinity. Now, while I admit that the finger introduced in the direction of the axis of the brim, as may be done in vaginal examinations, touches the cranium near the tuber parietale, I assert that, on the other hand, the finger, carefully introduced in the axis of the brim, touches the cranium in or near the sagittal suture, and at a point in its length varying according to the degree of flexion of the head. This is what Leishman effects by using Professor Buchanan's rectangular staff.

"*At the entrance of the pelvis* (says Nægele) *the head does not take a perpendicular, but a perfectly oblique direction, so that the part which lies lowest or deepest, is neither the vertex nor the sagittal suture, but the right parietal bone.*" Now, it is evident that, at the entrance of the pelvis, the head does not take a perpendicular, but a perfectly oblique direction. It does so because the whole child is lying obliquely; and to enter the brim of the pelvis directly—that is, in the direction of the axis of the brim—it must take a perfectly oblique direction. If it took a perpendicular direction—that is, a direction at right angles to the horizon—it would necessarily enter the pelvis with obliquity; but it takes an oblique direction in order to enter the opposed oblique brim of the pelvis directly, that is, in the direction of its own axis and of the axis of the brim.

Further, as Nægele says, the part which lies lowest or deepest is neither the vertex nor the sagittal suture, but the right parietal bone. All this is a necessary consequence of the direct entrance of the head. No doubt it may also be a consequence of the oblique descent of the head, but its occurrence is no proof of the obliquity. The direction of the head entering the brim is nearly that of a line striking the horizon at an angle of 30 degrees. This is a very considerable obliquity to the horizon, but is perpendicular to the brim of the pelvis, which is inclined to the horizon at an angle of 60 degrees. To enter the brim of the pelvis obliquely, the child's head must advance horizontally, or in a line of direction striking the horizon at an angle less than 30 degrees. Nægele does not say that the part which lies lowest or deepest in the brim is the right parietal bone. If he intended to say that, I am at issue with him; and appeal to observation in support of my assertion, that both parietal bones enter and pass the brim simultaneously, both being in the plane of the brim at the same time.

" *The sagittal suture* (says Nægele) *is much nearer to the promontorium of the sacrum than to the os pubis, and divides the os uteri, which projects backwards, and generally somewhat to the left, across into two very unequal segments.*" The position of the sagittal suture in regard to the promontory of the sacrum cannot be discovered by an examining finger, the parts being too distant to be reached in that way. When the hand is introduced into the vagina to feel the whole relations of the parts before the fœtal head has passed the brim, the sagittal suture is not found nearer the promontorium than to the pubic symphysis. I am thus, at this point, quite at variance with Nægele, and I may venture to point out the cause of his error. It is my opinion that it has arisen from not making the observations relied upon, while the fœtal head was at the brim of the pelvis, and then only; for, after the head has passed the brim and entered the pelvic cavity, the sagittal suture is generally found nearer to the sacrum than to the pubes: and this is not very unfrequently observed even before the os uteri is much dilated, or the labour has been long continued. This approximation of the sagittal suture to the sacrum arises from the descent of the head in the axis of the brim, which coincides with the axis of nearly the whole upper half of the bony pelvis. This axis, when prolonged, strikes the sacrum at or near its point. The fœtal head has a tendency to advance in this axis, and does so till it is arrested by the posterior wall of the pelvis. While it is passing between the symphysis pubis and the two upper bones of the sacrum, it has the sagittal suture equidistant between them: but afterwards, and until the head begins to advance more or less forwards, the sagittal suture approaches to the sacrum, as it descends in an axis which leads it in that direction.

The last part of the quotation just given from Nægele has very little value, for two reasons,—viz., because the situation of the os uteri is far from being fixed or invariable, and because no observations have been made even as to what is its most ordinary position with minute exactness. Nægele himself mentions a frequent deviation to the left, which has not received much notice from subsequent authors. While the head is at the brim of the pelvis, it is difficult to examine satisfactorily the relations of the sagittal suture to the moderately dilated os uteri; and the greater facility of reaching and examining the right and anterior than the left and posterior parts of the head naturally leads, and I believe has led, to exaggeration of the extent of the anterior half over the posterior half of the part corresponding to the circle of the os uteri. But it is out of place to pursue this particular point further, because its settlement would prove nothing, and lead only to the further question of the exact position of the os uteri in early labour, for the ascertainment of which we have no satisfactory data.

" *The higher the head is* (says Nægele) *the nearer its long diameter corresponds to the lateral diameter of the pelvis, and the more oblique is its direction; from which reason the right ear can generally be felt behind the pubis without difficulty, which would not be the case if the head had a perpendicular direction.*" In regard to the increase of obliquity according to the height of the head, I have not one word to say, except that it is a mere statement on Nægele's part, unaccompanied by any corroborative evidence. As I do not believe in the obliquity at all, I can find no place for this refinement.

Nægele's assertion regarding the right ear is quite as much in accordance with the theory of the direct entrance, or entrance in a perpendicular to the brim, as with his own theory of obliquity. I must confess myself completely at a loss, however, as to the full bearings of his argument; for I know well that under no circumstances is the right ear felt behind the pubis without difficulty, and that its position when felt, and the forcing of the finger between the pubes and the head in order to reach it, indicate the direct, not the oblique, entrance of the fœtal head into the brim of the pelvis.

It is worth while here to remark that Stoltz, who supports the doctrine of Nægele, takes care to guard himself from being held as agreeing with the Heidelberg professor in his statement that the higher the head the more oblique is its direction. He says,—"Je n'ai jamais trouvé, malgré toute mon attention, que la tête fût d'autant plus inclinée qu'elle etait plus haute; encore moins ai-je jamais pu sentir l'oreille du fœtus aussi longtemps que la tête était au-dessus du détroit superieur." *

III. The third reason for rejecting the theory of obliquity at the brim of the pelvis is based upon a careful study of the production of the caput succedaneum, and of the relations of this swelling to the presentation,—a subject in regard to which much has been assumed without evidence, or in defiance of it.

"*Under certain circumstances* (says Nægele), *a swelling of the integuments of the head frequently forms soon after the os uteri has begun to dilate. . . . This swelling is situated upon the right parietal bone, close to its upper edge, and equally distant from both angles: a small piece sometimes extends over the sagittal suture unto the other parietal bone; its circumference depends upon the degree of dilatation*

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* *Considérations sur quelques points relatifs a l'Art des Accouchements*, p. 20.

which the os uteri had attained." This statement of facts by Nægele may be added to, but cannot, I believe, be controverted. The caput succedaneum of the first stage of labour can be felt and seen to be as described in the passage. The statements in the quotation immediately preceding this last, and in others, is in quite a different position, involving points not of easy observation, and in regard to which there are manifest inducements to error. But, while I agree entirely with Nægele's statements in this last passage, I would make two additions thereto, of which only the second has an important bearing on the subject of this memoir.

First, It is a condition necessary for the formation of a true caput succedaneum, that the liquor amnii be evacuated, or that it be in such minute quantity as to have no hydrodynamical properties. *Secondly,* While the extent of the caput succedaneum of the first stage may be limited to the right parietal bone, it is generally so limited only when it is not well developed. When it is well developed, it is found to extend over the upper part of the left, as well as of the right parietal bone; but its greatest thickness is, as a rule, always in the portion overlying the right parietal bone. Nægele himself mentions the extension of the swelling over the left parietal bone as an occasional occurrence of which he gives no explanation.

Before further advancing, it is necessary to inquire what evidence is derivable from the caput succedaneum. In answer, it is certain that it only indicates what was the unsupported part of the head,—in the present instance, what part lay over the os uteri. And as the position of the os uteri is uncertain, and denotes nothing exact topographically, so the position of the caput succedaneum will denote nothing exact topographically, or relative to the position of the fœtal head in the brim of the pelvis. For a fuller development of this subject I beg to refer the reader to the following chapter.

But, before leaving this point, I think it advisable to show that, supposing, as Nægele seems to do, that the os uteri occupies exactly the centre of the brim (except slight deviation to the left), and that the caput succedaneum formed on the part of the head lying over it marks the part lying in the centre or axis of the brim, the indications afforded by this swelling are not truly read off; and that, if truly read off, they indicate direct, not oblique, entrance of the head into the brim.

The caput succedaneum of the first stage of labour is often formed after the head has passed the brim of the pelvis, and is lodged in the upper half of the cavity of the bony pelvis. Were we to be cautious and exact in reasoning, all such swellings should be excluded from the argument, for evident reasons. It is only those formed at the plane of the brim, or very near it, that can, under any circumstances, afford assistance in settling this question. Under the actual deficiency of exact data, we must be content with stating principles. Now, it is evident that the direction of the caput succedaneum of the first stage will be that of least resistance—that is, the direction of the axis of the undilated vagina ; in other words, the caput will be thickest where the head is least supported, and may, in other parts within the circle of the os uteri, be so inconsiderable as not to attract notice. Further, and for the same reason, the centre of the caput succedaneum, or the centre of the os uteri, will not correspond with the thickest portion of the swelling, but in this case be behind it, or nearer the left parietal bone. The oblique direction downwards and forwards of the vagina will lead the caput in that direction ; and the support given by the posterior wall of the vagina to the posterior half of the space enclosed in the circle of the os uteri will cause thickness of the swelling over the right, and comparative thinness over the left parietal bone, and displacement of the thickest portion of it forwards in the pelvis—that is, in the direction of the right parietal and away from the left parietal bone.

IV. A very cogent argument against the existence of this lateral flexion of the child's head, or obliquity to the plane of the brim, at the commencement of labour, is derived from the impossibility of finding a mechanism to account for it.

If the membranes are still entire, and there is present any considerable quantity of liquor amnii, and if the axis of the child and uterus are parallel to or identical with the axis of the brim, all which conditions are usually found, it is impossible to conceive any cause of the obliquity but a spontaneous lateral flexion of the child's head; and I daresay no obstetrician will support so extraordinary a doctrine as that the child should, without any discoverable cause, and I may add without any desirable object, bend its head towards its left shoulder as it begins to pass the brim.

If the liquor amnii has been evacuated before the fœtal head has entered the brim, or if the liquor amnii be very scanty in amount, then forces produced by the pains, or the lower parts of the womb and adjacent structures, may be imagined to act directly upon the child, and cause the obliquity. But although they may be imagined, they do not, I believe, exist. If the uterus, for example, became, during pains, more nearly horizontal than it ordinarily is, or quite horizontal, then this grave anterior obliquity of the uterus would probably cause the head to present indirectly or obliquely at the brim. But the opposite of this is observed in nature. The uterus, during a pain, becomes, as it were, erect, and to a certain extent corrects any obliquity it may have during relaxation, becoming, when in contraction, perpendicular to the brim of the pelvis,—that is, occupying its axis. It is certain then, that no anterior uterine obliquity is observed, which might account for the obliquity of the head at the brim. And it is necessary to remark that the obliquity at the brim, if supposed to be produced by anterior obliquity of the uterus, would not be accompanied by lateral flexion of the head as a change in the fœtal attitude. The flexion of the fœtal head which is so often observed in the early part of labour is easily accounted for by the circumstance that the fulcrum of the head—the spinal column—is nearer the occiput than the sinciput; and all forces acting equally on the various portions of the vertex, act with advantage on the sinciput, as it is at the end of a longer lever than the occiput. But in the case of the sides of the head, the right against the left tuber parietale, no such inequality is observed.

In short, no mechanism has been devised to account for the phenomenon, and it is a vain pursuit to seek it, at least on my part, as I deny the existence of what is to be accounted for.

I must not omit, however, to state that Stoltz has suggested an explanation of the occurrence of lateral flexion. But to me it is so unsatisfactory as not to lead me to modify the statements I have made. His words are:—

"Mais quand les contractions de la matrice sont fortes et qu'elles agissent puisamment sur le corps qu'elle renferme, quand la tête s'engage dans l'entrée du bassin, alors son inclinaison devient plus grande; la suture bipariétale s'éloigne peu à peu du centre du détroit, et se retire en arrière et en haut; la bosse pariétale droit se découvre presque entièrement, principalement à l'endroit où elle correspond à l'angle postérieure et supérieure de cet os, et cette flexion latérale de la tête ne fait qu' augmenter jusqu'au moment ou elle và franchir le détroit supérieur. Voici comment je m'explique cette disposition. Au commencement du premier temps du travail les contractions n'agissent pas encore fortement sur le fœtus; il peut conserver sa position droite; mais quand une fois la tête s'engage et eprouve une grande résistance, le col, qui est la partie la plus faible du tronc, cède; les contractions n'agissent plus d'après une ligne droite, qui, du centre de l'extrémité qui se trouve au fond de l'utérus, irait à peu près au milieu du sommet, ce qui incline la tête de côté, et fait qu'on n'atteint plus alors qu'avec plus ou moins de difficulté la suture sagittale qui est dirigée en arrière. C'est alors que, située obliquement par rapport au détroit du bassin, elle présente la pariétale plus bas que la suture sagittale elle-même." *

V. Assuming that the fœtal head enters the pelvic brim obliquely, Nægele claims for this condition a mechanical advantage over the direct entrance. "*On account,*" says he, "*of the oblique position of the head, the greatest width of the cranium (from one tuber parietale to the other), as well as that of its basis, can never, during its passage, coincide with the diameters of the pelvic entrance.*"—(P. 19.)

It is necessary, *in limine*, to state that discussion on this point, and conclusions in regard to it, can lend no aid to the settlement of the question under consideration here. It is, indeed, quite a work of supererogation to consider at all the advantages presented by an oblique or direct entrance of the head into the pelvic brim, until the previous question be settled, whether the entrance be direct or oblique.

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* *Considerations sur quelques points relatifs a l'Art des Accouchements,* p. 20.

The position apparently offering the greatest mechanical advantages is not always adopted by nature. Mechanical difficulties seem in various points to be sought, instead of mechanical advantages. The whole process of labour is, indeed, beset with difficulties, one of whose objects is, without doubt, to prevent its too easy and rapid accomplishment. One example, germane to the subject of this article, I may adduce from the entrance of the fœtal head into the pelvic brim. It is well known that its entrance, considerably flexed, has a great mechanical advantage over its entrance slightly flexed or not flexed at all; yet, in spite of this mechanical advantage on the side of the greater flexion of the head, we find that it generally passes the brim slightly flexed or not flexed at all.

Nægele places the mechanical advantages of the supposed obliquity entirely in the dimensions presented to the plane of brim by the transverse diameters of the cranium and of its basis. In including the basis in his statements, he is decidedly wrong. He would, indeed, appear to forget that the fœtus has a neck, the addition of which to the basis, even when the head is laterally flexed, makes the direct entrance of the basis mechanically the most advantageous, and that so evidently that it is really needless to do more than assert it.

There is no doubt that if the fœtal head passes the brim directly, the greatest biparietal diameter (from

one tuber parietale to the other) passes it, and th
the head enters the brim with obliquity, a sm
biparietal diameter supplants the greatest,—tha
in the first position, a diameter measured from b
the right tuber to above the left. But Nægele se
for the moment, to have forgotten that the
diameter of the fœtal head is not in the trans
diameter of the brim, but in an oblique diamet
this part, and that consequently (as well as for
reasons), the diameters of the fœtal head whic
wishes to be compared are not the diameters
necessary to compare, for they do not pass the sma
diameter of the brim. If mechanical advantage i
way of dimensions is to be of service in the mechan
it must meet the difficulty,—that is, the gai
diminution of dimension must be in the part traver
the conjugate, or small diameter of the brim; and
is not true of the gain spoken of by Nægele. Bu
truth, no gain is desiderated in any natural case,
when the comparison of the oblique and direct
meters of the part of the head traversing the conju
diameter, as suggested by Nægele, is made so as t
true to the mechanical conditions, it is found tha
appreciable gain is got from obliquity. This
parison is too difficult to give in words so as t
useful to a reader. It is necessary to institute it
the fœtal head and callipers in the hands in ord
verify it.

There are, however, mechanical conditions of
laterally flexed head, or of the head presenting

parietal bone to the brim, which, if such a position were assumed, would lead to great and perhaps insurmountable difficulties in a labour. For, if the vertex was by any arrangement displaced from its position at the brim, as Nægele describes, it would tend always to be more and more displaced, till an ear, or even a shoulder, descended. And if the canal of the uterus were rigid and contracted enough to resist such unnatural dislocation, the uterine efforts would be directed along the body of the child to its head, at a great disadvantage. Other mechanical evils might be suggested; but it is in vain to raise difficulties, which, if the object of my memoir is gained, are all chimeras.

CHAPTER V.

OBLIQUITY OR LATERAL FLEXION AT THE OUTLET.

WHILE I hold it proved that the child's head passes through the pelvic brim directly, I have, before leaving the subject, to point out that, after it has passed through the brim and upper half of the ligamentous pelvis, it does advance obliquely in its subsequent progress,—that is, the head of the child impinges on the planes of the parts of the pelvis through which it is passing, or on planes at right angles to the axes of the lower parts of the pelvis, not directly, but indirectly or obliquely. A mesial part of the fœtus does not first touch these planes, but a lateral point.

Leishman has pointed out that Nægele somewhat vaguely describes this lateral flexion. Stoltz, who adopts Nægele's view as to lateral flexion at the brim, describes it unmistakeably. He says:—

"Cette manière de se dégager du détroit inférieur, ressemble infiniment à celle suivant laquelle la tête s'engage et franchit l'entrée du bassin. Elle présente d'abord une des extrémités du diamètre transverse et une du diamètre antéro-postérieure, la bosse pariétale et l'occiput, et seulement quand ces deux sont sortis, les deux autres s'engagent et sortent à leur tour: c'est la manière la plus favorable possible. Il est pourtant juste de dire que quelquefois il se fait un véritable mouvement de rotation, telle qu'on l'a décrit; mais ce sont des exceptions, qui n'ont lieu que sons l'influence de causes particulières; qu'il serait trop de détailler ici." *

In the first half of the head's course through the ligamentous pelvis, a point in or near the sagittal suture is the presenting point. There the caput succedaneum is formed. During this part of its course the head advances in the axis of the brim, which almost exactly corresponds with the axis of the upper half of the ligamentous pelvis. During all this time, if the head is not covered by the cervix uteri, the right parietal bone is the part first and easiest felt; and the further it advances, the more is this the case. As it advances, and passes the first bone of the sacrum, the sagittal suture approaches nearer to the sacrum, or rather to its lower portions, and becomes more distant from the symphysis pubis. When the biparietal diameter of the head has reached the lower boundary of the upper half of the pelvis, it is arrested in its direct progress. The vertex impinges on the posterior wall of the pelvis, and in its further advance, the head, as a whole, must change the direction of its course. This change of course is too abrupt for the parts of the head to follow it perfectly. Moreover, there is no room in the pelvis for such a degree of

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* *Considerations sur quelques points relatifs a l'Art des Accouchements*, p. 25.

lateral flexion as this would imply,—that is, as would be necessary to maintain the head presenting directly to the plane of the pelvis through which it is now passing. While advancing at this point of its progress, the presenting part, therefore, is changed. It soon becomes the upper and posterior part of the right parietal bone, instead of, as before, a point in the mesial line of the head. With this point advancing in the axis of the pelvis, it is evident that the sagittal suture or mesial parts are far removed from it, and consequently that the head is passing through the lower half of the pelvis, the outlet, over the perineum, and through the vulva, more or less obliquely, and not directly. In accordance with this obliquity, the child's head is flexed laterally, or to be more exact, flexed obliquely,—that is, bent not directly over the right shoulder, but in a direction midway between extension and direct lateral flexion. As it approaches the orifice of the vulva, and rotates so as to bring the occiput nearer to the pubes than it was in the earlier parts of its progress, this flexion gradually approaches nearer to extension; but it does not become direct extension, almost always maintaining an obliquity,—that is, a direction between extension and flexion.

Under two sets of circumstances, not observed in ordinary labours, the presentation of the fœtal head may be direct from the beginning to the end of the process. In the rare cases where the head enters the brim and passes through the whole pelvis with its long axis in the antero-posterior vertical plane of the passage, the head will offer itself not obliquely, but directly, in its whole course. The presentation, indeed, will only shift backwards upon the child's head as it descends, maintaining always a position in the mesial line. Again, it is possible that the fœtal head may descend directly with its long axis in the transverse vertical plane of the pelvis, till it makes a complete quarter of a circle rotation, bringing it into a direct antero-posterior position. Such cases are not subjected to the ordinary laws of the mechanism of parturition.

It is to be remarked, then, that in the second half of its progress the head does not present directly, but obliquely, and that it is born with this obliquity. But this last obliquity is unlike the former, in being quite in accordance with Næegele's statement of the phenomena, though he omits to name it lateral obliquity; the obliquity which he names and describes in this portion of labour being the position of the long axis of the head in the right oblique diameter of the pelvis, not the oblique presentation of the head to the plane of the outlet, or other portions of the pelvis through which it passes in the latter parts of its course. Further, not only is there observed this obliquity to the planes of the pelvis, but there is a change in the attitude of the fœtus simultaneously produced. The head is at first, in this second part of its course, laterally flexed to the right with a backward obliquity; and when passing the vulva this is slightly changed, the condition being one of extension, with a lateral obliquity to the right shoulder.

This obliquity of the child's head to the planes of the lower parts of the pelvic passages is not only observed, but is easily explained. In its descent, the head, if of its ordinary size, must follow the direction of the curved axis of the pelvis. It is possible to imagine the presentation continuing direct while the other parts of the mechanism remain unchanged; but there is no room in the pelvis for the great right lateral flexion of the head that would be necessary to maintain the presentation direct, and the mechanism does not demand it. A certain amount of lateral flexion is made, and this diminishes the obliquity. This moderate lateral flexion is not produced by spontaneous fœtal motion, but by the powers of labour urging the child through a canal which at this part is rigid and contracted enough to force the soft fœtus to adapt itself to its graduated curvature.

CHAPTER VI.

ON THE CAPUT SUCCEDANEUM, THE PRESENTATION, AND THEIR RELATIONS IN CASES WHERE THE HEAD COMES FIRST.

I HAVE found so much indefiniteness or error in the subject matter of this chapter, that I have thought it necessary to attempt to give some precision and clearness to the terms "Presentation" and "Caput Succedaneum," and to show some of the relations which the one bears to the other. I hope to convince my fellow-students of the necessity of adopting the views I now submit; and I have no doubt of their at least agreeing with me in feeling the need there is of exact definition of terms in such discussions as those concerning the mechanism of parturition. If I do nothing more than explain my own views, I shall have the advantage of starting from them as standpoints in future writings, and of more easily carrying with me the intelligence, if not the convictions, of my readers.

It is apparent that, before describing the relations of one thing to another, it is necessary to define clearly what these things are—in the present case, what the caput succedaneum is, and what the presenting part is.

With a view only to the present discussion, it is not needful to describe fully the nature and origin of the caput succedaneum. It is defined sufficiently as a swelling which may be produced during labour in the scalp of the fœtus. When it occurs, it is not formed indiscriminately upon any part of the cranium of the child, but upon those parts which are little supported or altogether unsupported by the structures forming the maternal passages. In the course of labour, after the evacuation of the liquor amnii, the child is during pains subjected to strong pressure from the parturient forces, and equally strong counter-pressure from the resisting maternal passages. Every part of the child is subjected to these forces, except that adjacent to the as yet undilated passage through which the child is being urged. This last suffers in some parts no counteracting pressure, and in some it may have only a modified degree of it, and upon all such parts the swelling may be formed.

But the caput succedaneum is very incompletely defined unless its direction is observed, as well as the influence of this direction in modifying its shape,—in other words, in giving it different degrees of thickness in different portions of the area upon which it is formed. Now it is evident that this direction will be the direction of least resistance. It will not necessarily be the direction of the propelling forces, nor the direction in which the child will subsequently advance, but it will necessarily be the direction of least resistance to it at the time of its formation. The caput succedaneum of the second stage of labour is a good example of its direction not being that of the propelling force. The caput succedaneum of the first stage of labour, with the head at the brim of the pelvis, is often a good example, though not so striking as the former, of its not adopting the direction in which the child will make its next advance.

If we keep in view the area upon which the caput succedaneum is formed, and the direction in which it projects, it will be easy to understand that its mass and thickness will be greatest in the line of projection. Obstetric authors evidently assume tacitly that the thickest or most prominent part of the swelling corresponds to the centre of the area upon which it has been formed, and this is a mistake leading to further erroneous conclusions. No doubt, if the direction of least resistance be a line at right angles to the centre of the unsupported area, then the thickest portion of the caput succedaneum will indicate the centre of the unsupported area. But if the direction of least resistance be a line at right angles to the unsupported surface of the child's head, but not passing through its centre; or, if it strikes this surface at an acute angle (as generally occurs in the history of the caput succedaneum of the first stage of labour), then the greatest thickness of the swelling may be very far from corresponding to the centre of the unsupported area on which it is formed (that is, in the case of the caput succedaneum of the first stage, the centre of the os uteri).

The term presentation is used by authors to signify very different and ill-defined things. With some it means the whole part of the head that can be reached by the finger, or whose topical relations are defined by the term position. By others it is used to imply the part of the head first reached by the examining finger. By others again it is employed simply to mean the part coming first.

In Dr. Tyler Smith's *Manual* a passage occurs which indicates a feeling of need of definiteness on this point:—" It is necessary," he says, " that what is meant by the presenting part of the fœtal head should be clearly defined. Hitherto a good deal of confusion has prevailed upon this subject. Is it the part found most prominent within the ring formed by the soft parts of the parturient canal in the different stages of labour—namely, the os uteri, the vagina, and the ostium vaginæ? Is it the part of the head found lowest in the pelvis during the progress of labour? Or is it the part first met with on introducing the finger into the pelvis in the direction of its axis? It will be found that all these points of view are mixed up together in some of the best and most recent works on obstetrics, with the effect of causing considerable uncertainty. For instance, the right tuber parietale is very commonly said to be the presenting part in the first and fourth positions, as the head passes through the brim and upper part of the pelvis, because it is the lowest point met with on introducing the finger into the vagina. It is, however, more frequently—

indeed, almost invariably—felt through the anterior wall of the cervix, and not within the ring of the os uteri, unless after the full dilatation of the latter. I would suggest that it would be best to define the presenting part in every kind of cranial position or presentation, as that portion of the fœtal head felt most prominently within the circle of the os uteri, the vagina, and the ostium vaginæ, in the successive stages of labour."—(P. 282.)

I regret that I cannot adopt Dr. Smith's definition, simply because it is not more definite than its predecessors. For we have no explanation of what is meant by the most prominent part; and if we had a definition, we could not ascertain it with any certainty; and further, were it ascertained with certainty in any case, it would be a part having no important mechanical relations to anything; or, in other words, no fixed relations to the passages in the mechanism of parturition. The definition I submit in this paper is at least certain, and fixed, and important. Although it may be in any case impossible to make it out exactly, it is always to be made out approximatively, and is the centre of the advancing surface of the child. It is, at least, capable of being always understood, if not marked on the actual surface of the child.*

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* On this subject Leishman has the following reflections, which I introduce with the remark that it appears to me that it is our duty to seek scientific accuracy, to seek good definitions, quite as much when we cannot in practice adhere to them as when we can. "As regards the word Presentation," says he (*Mechanism of Parturition*, p. 55), "the difficulties are even greater, and the confusion among various writers

It will be apparent, on reflection, to all obstetricians, that this vagueness can no longer be allowed to rest upon the useful and important word presentation. At present it is generally used to mean the part first reached by the finger examining per vaginam, and passed in the direction of the axis of the pelvis. This is the meaning of it in Nægele's writings; and this definition, if modified so as to be exact and incapable of

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more embarrassing. If we imagine a series of planes radiating from a common centre in front of the pubes, and passing to the posterior part of the pelvic wall, the upper of these being the plane of the brim, the lowest that of the outlet, the parabolic curve which passes through the centre of them is manifestly the axis of the pelvis. Now, if we are forced to name a *point* as the presentation, we might, with some show of propriety, adopt that definition which limits the presentation to that point on the surface of the child's head through which the axis of the pelvis passes.—(Dr. Matthews Duncan, *Edinburgh Medical Journal*, 1861.) But the objection to this is to be found in the fact, that it is practically impossible to determine this with anything approaching accuracy, and therefore we must adopt, for our information and guidance in practice, some more simple, if less accurate, idea of the term. This being, then—so to speak—beyond the pale of scientific accuracy, every one has been left to attach his own interpretation to the phrase; some regarding it as the part lowest in the pelvic cavity; some as the part which the finger first touches when introduced in the axis of the outlet; and others as the part which is lowest with reference to the plane of that part of the cavity in which the head is situated. Of these the last is the most correct. Dr. Tyler Smith, who seems to have recognised both the difficulty and importance of clearly understanding what a presentation is, suggests "that it would be best to define the presenting part, in every kind of cranial position or presentation, as that portion of the fœtal head felt most prominently within the circle of the os uteri, the vagina, and the ostium vaginæ, in the successive stages of labour." This definition is so far faulty in overlooking the relation which the head bears to the bony pelvis, but it is nevertheless the most correct and intelligible of any that I have seen."

being misunderstood, will, I believe, be found satisfactory. It might be wished to reduce the presentation to a point instead of a part. But, at present at least, in view of the mobility of the head in the pelvis, and on the neck, and considering the difficulty of arriving at nice precision, the presentation, while regarded as a point, may be spoken of in practical discussions as a part. The only definition, having sufficient exactness, which I can suggest, is that it is that point on the surface of the child's head, or other part advancing first, through which the axis of the pelvis passes. It is evident that this corresponds to the part first touched by the finger, if passed in the axis of that part of the pelvis in which the presentation is lying. But if the presentation is high up, no finger can do this; and wherever the presentation may be, no accoucheur can, groping with his finger, fix very exactly the position of the imaginary line forming the axis of the pelvis. The direction of the axis may be more easily found than the axis itself.

It must be remarked that, while the presenting point is frequently the lowest in the pelvis, it is not necessarily so; that while it is frequently the part first reached by the examining finger, it is not necessarily so; that while, in the first stage of labour, it frequently corresponds to the area encircled by the os uteri, it is not necessarily in that area; and that, while in the course of the second stage of labour it frequently corresponds to the lumen of the vagina, it does not necessarily do so. In all of these cases it is for the same reason that the presentation and the various parts named are not always identical. The reason is, that the various parts named are occasionally not in the axis of the part of the pelvis occupied by the advancing part.

The definition just given of the term presentation has the very great advantage of adapting itself to the fundamental writings of Nægele on the mechanism of parturition. These writings will, indeed, be found easier understood if the definition above given be assumed as that which Nægele would have given had he addressed himself specially to the point. It is to be regretted that he did not; for, so doing, he would have probably more carefully considered the value of the caput succedaneum as an index of presentation, and thus avoided some inaccuracies, if not errors, which this deficiency has led to in his admirable work.*

In order to facilitate the comprehension of what has now to be said regarding the relations of presentation to caput succedaneum, it will be well for the reader to suppose that the head is presenting in the usual way. The general principle as to their having no absolute or necessary connection with one another, has already been stated. To apply this principle, and show its influence at different times, still remains.

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* The following passage from Dr. Churchill's manual is cited in illustration of the present method of treating this subject. In the opinions expressed by Churchill he is supported by Nægele, Hohl, Scanzoni, Cazeaux, and Braun, as well as by British authors.

Speaking of the diagnosis of cranial presentations, Dr. Churchill says (p. 196, Edition 1860);—"We possess an unfailing test of the correctness of our diagnosis in the tumour of the scalp, or 'caput succedaneum,' as it has been called. It is formed by the pressure of the head against the openings through which it has to pass—*i.e.* first against the circle of the os uteri, and secondly against the circumference of the vaginal orifice; and it always forms on the lowest or presenting part, so that the primary tumour indicates the part of the head which presented at the os uteri, and the primary and second together, that which occupied the lower orifice."

If the lumen or caliber of the maternal passâge be small, then the unsupported area of the child's surface, or the part not pressed by the resistance to the child's progress, will be small, and the caput succedaneum will occupy a similarly small extent of surface; and *vice versa.*

In the first stage of labour, before the os uteri is much dilated, the caput succedaneum, if present, always corresponds to the os. Wheresoever the os uteri may lie, there is the caput succedaneum. The caput succedaneum may or may not correspond to the presentation. It will always correspond with it, if the axis of the part of the pelvis in which the os lies passes through the centre of the os. When the os is at or near the brim of the pelvis there is no doubt that, under standard conditions, this is the case; and consequently the caput succedaneum of the first stage frequently coincides with the presenting part at that time. In the first stage of labour, before the os uteri is fully dilated, and while it is at or near the brim, the direction of the caput succedaneum corresponding with the direction of the vagina will always be oblique;

that is, it will not strike the head at right angles to the presenting part, but at an acute angle opening towards the anterior parts of the mother. This corresponds pretty nearly to the direction of the index finger, as ordinarily employed in vaginal examination. As a result of this oblique direction, the caput succedaneum will be much thicker over the anterior portions of the presenting surface than over the posterior,—that is, over the parts near the anterior lip of the cervix than over those adjacent to the posterior. And this is a matter of common observation.

In the latter part of the second stage of labour, when the perineum is distended, and the presenting part, having passed the outlet of the ligamentous pelvis, is presently to pass through the orifice of the vulva, the caput succedaneum then formed will necessarily correspond very closely with the presentation, as is daily observed. The reason of this is obvious, the unsupported part corresponding to the part which is directly to pass the external orifice of the woman's body. At last the centre of the caput succedaneum will be the presenting point, as it will correspond to the axis of the vulvar orifice, and as there is no resistance to the projection of the swelling in any direction.

In the course of the second stage, after the complete dilatation of the os, and while the presenting part descends more or less in the axis of the superior strait, the relations of the caput succedaneum and presentation will approximate to those described as existing between them in the first stage of labour. In the course of the second stage, after the head has begun to advance more or less in a direction forwards, the presentation and caput succedaneum will generally very nearly coincide, as has been described to be the case in the latter part of the second stage.

CHAPTER VII.

THE PRODUCTION OF INVERTED UTERUS.

My principal object in this chapter is to throw light on the subject of the mode of production of inversion of the uterus, and especially to establish the theory of its causation in cases which more than others deserve the name of intussusception, in which the uterus is inverted, not merely spontaneously, as the phrase is, but inverted by its own active efforts.

In pursuing this object, I shall not describe the phenomena of any individual cases of this rare disorder. I have met with instances of the accident, and may, at another time, record them. They would only encumber me at present, because no individual case has other than very limited bearings on the extensive subject to be discussed. Although no individual case is of much value, yet the whole knowledge which we have of the affection is founded on individual observations, and it is this knowledge which forms the basis of all the reasonings to be now detailed.

In the outset, and to give some previous idea of what is to follow, I here state that much illumination of the subject has arisen to myself from regarding retention of the placenta by uterine spasm or stricture, generally called in this country, hour-glass contraction of the uterus, as the affection most closely allied to inversion of the uterus produced by uterine action. Holding that hour-glass contraction is a disorder nearly identical in its first stage with the first stage of spontaneous active uterine inversion, I shall, I hope, be able to make these two conditions throw light, one upon another, so as to establish the true theory of both.

It will be useful here to define the terms qualifying inversion which I propose to use. By passive and active, I imply uterine passivity or activity, not passivity and activity generally; and uterine activity necessarily implies muscular uterine action. By spontaneous and artificial, I indicate the absence or presence of forces external to the patient. Spontaneity implies that forces in the organism of the patient effected the inversion. Artificiality implies that the production of the inversion was partly or wholly the result of some kind of mechanical interference by the accoucheur, or even by the patient herself.

I shall not enter into historical details regarding this subject. Many authors, British and foreign, have made advances towards what I regard as the true theory of uterine inversion. Paralysis, or non-contraction, or weakened action of parts of the uterus, with contraction or spasm of other parts, has been, by many authors, pointed out as contributing to an understanding of the phenomena of encysted placenta, or hour-glass contraction. And there we have the germ of the theory I now propose.

Levret, speaking of one of his drawings, says—"It gives an idea, though superficial, of what sometimes happens when the placenta is encysted instead of separated from the uterus after the birth of the child, which is generally a consequence of inertia of the part of the uterus on which the placenta happens to be implanted."* In another place, describing a case of encystment, he speaks of the contracted condition of the uterus "except in the site of the attachment of the placenta, which was persisting in a state of inertia, and had become the formal and occasional cause of the uterine cell which imprisoned, so to speak, the placenta. . . . This was also the cause of the close adhesion of the placenta to the portion of the uterus on which it happened to be implanted." . . †

"The neck of the uterus," says Lachapelle, "is often inert, although the fundus is contracted; sometimes the reverse happens, and it is then that the placenta, enclosed in the uterus, appears to be encysted in it."‡

Hegar, in his work § on retention of the placenta, makes the following statements:—"According to Seiler, the cause of the incarceration is the activity of the uterus in those parts to which the placenta is not attached, along with inaction of the seat of its insertion. . . . According to Desormeaux, the enclosure of the placenta arises from this, that the part to which it adheres does not make progress in its contractions equal to that of the other parts." These extracts justify what I have said in a former paragraph. I know no author who has, to an equal extent, advanced the subject of the causation of active uterine inversion.

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* Fig. 15, vol. ii.—*L'Art des Accouch.* 1761, p. 294.

† *Observations sur les causes et les accidens de plusieurs acc. labor. Suite des observations.* Obs. xxvii. p. 132.

‡ *Pratique des Accouchements.* Tome ii. p. 378.

§ *Die Pathologie und Therapie der Placentarretention*, etc. Berlin, 1862. S. 30.

But although no author has, so far as I know, founded a theory of active inversion on paralysis of the uterus at or about the site of the placental insertion, yet, in recent times, there has been an increasing amount of support given by authors to the opinion that inversion is frequently produced spontaneously by morbid uterine action. Formerly, the chief place in the causation of the disease was given to mechanical interference, and generally injudicious or ill-executed interference by the accoucheur. This change, whether well-founded or not, is a step towards what I regard as the true theory of spontaneous active inversion. Yet I shall, subsequently, show that, so far as I know, it has led only to erroneous speculations as to the nature of the phenomena.

I may here say that I am inclined to think that the above-named change in opinion has probably gone too far, as I believe that pulling the cord or otherwise removing the placenta, and other mechanical interferences, whether well or ill done, may, in the condition of general uterine relaxation, often be the only assignable cause of inversion; and in the condition of irregular uterine action, it may be a part of the cause leading to depression of the placental insertion into the uterine cavity, changing the case from one of uterine spasm, with encystment of the placenta or hour-glass contraction, into one of commencing inversion, producing thus the first stage of complete inversion. To this view, as to the change of opinion, some facts lend support, such as the frequency of the affection in unskilled hands, and its rarity in such institutions as the Lying-in Hospital of Dublin.* But I am not prepared to demonstrate what is my impression, and it forms no part of my present object to do so. I shall only further quote a passage from Denman to show that he believed mechanical interference to be sometimes the cause of a beginning of inversion which was subsequently completed spontaneously or by uterine action, an opinion in which I concur:—"I have," he says, "been assured that in some cases there has been a spontaneous inversion; that the accident happened at least when no force, or none capable of producing the effect, had been used; and then it was imputed to the shortness of the funis, giving the disposition before the birth of the child; or to some untoward action of the uterus. But with this assurance, or explanation, I do not feel quite satisfied, because degrees of force must always be vaguely estimated; though if a disposition to an inversion be first given by the force used in pulling by the funis, it may be completed by the action of the uterus; or, if the least possible degree of inversion were given by the shortened funis, it might certainly be completed by a very slight additional force in pulling by the funis, or by the mere weight of the placenta."*

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* "No example," say Messrs. Hardy and M'Clintock in their work on *Midwifery and Puerperal Diseases*, p. 223, "of acute inversio uteri has ever fallen under our notice, and the accumulated experience of Drs. Clarke, Labatt, Collins, Kennedy, and Johnson, in the hospital, does not furnish a single instance of the occurrence of this accident, though the number of women delivered during their united masterships amounts to upwards of seventy-one thousand." But since the work alluded to was published, a case has occurred to Dr. Denham, the present Master of the Hospital.

Among recent authors known to me, Drs. Radford and Tyler Smith appear to me to have arrived most nearly to the true theory of spontaneous active uterine inversion. Both collate the affection with irregular uterine action; Tyler Smith distinctly compares it to hour-glass contraction; and both speak of irregularity of uterine action as comprising a weakened action in parts of the uterus and strong action in others, or, in Tyler Smith's words, both spasm and inertia. Doing so, had they at the same time kept in mind the view of Seiler and Desormeaux as to the condition of the uterus in cases of incarcerated placenta, a view which they themselves apply in some cases of such incarceration, they would, no doubt, have arrived at the true theory of spontaneous active uterine inversion.

Radford † believes that action of the inverted part, with want of contraction of parts below it, is the theory of active uterine inversion. "There are," says Radford, "several varieties of irregular uterine contraction, and in all, some part of the organ must be in a state of weakened action, whilst another is in a highly excited condition. It is not essential to inquire here which of these two states lays the foundation of the mischief. The uterus is divided, in hour-glass contraction, into two compartments; in the upper the placenta is uniformly found. A contraction sometimes takes place in the body and cervix, leaving a chamber at the fundus, in which also the placenta is always found. There, again, occurs a contraction in the os uteri, with deficient action in the body and fundus. This condition leads to a retention of the placenta, and in some instances it takes place after the placenta has been removed, giving rise to internal flooding. There are states of the uterus where some portion or portions contract with greater force than the rest. Now, it appears to the writer that inversion is another instance of irregular contraction, in which the fundus acts powerfully, whilst the cervix and os uteri are in a state of atony. It is obvious that if the fundus and body continued their action after the expulsion of the child, before the cervix and os uteri have regained their proper powers, an inversion must take place."*

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* *Introduction to the Practice of Midwifery.* Waller's edition, p. 421.

† Saxtorph is said by West to be the originator of Radford's theory. But I have not been able to satisfy myself of this. Saxtorph's words appear to me to be too vague to justify the ascription of the theory to him. See West's *Diseases of Women*, 3d edition, p. 225. Besides, the statement of Desormeaux and P. Dubois (*Dict. de Médecine*, tome xxx. p. 354) seems to transfer the credit to Astruc.

To comment on this opinion of Radford fully would lead to largely anticipating what is better deferred to another part of this chapter. I shall, therefore, now only say, that I cannot conceive on what grounds Radford can support his view. For the powerful action of the fundus, whilst the cervix and os uteri are in a state of atony, is a natural, ordinary, healthy concourse of conditions. They appear to me to be the conditions which best secure the female against the dreaded accident; and I cannot imagine the production of it while they last. If Radford attaches special virtue to the word "powerful" in his assertion, it would only lead, so far as I can see, to clearer perception of the impossibility of inversion while the conditions last.

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* Essays on various subjects connected with Midwifery: *On Inversion of the Uterus*, p. 16.

The only kind of uterine inversion consistent with Radford's conditions of powerful contraction of the fundus, and relaxation below that part, is inversion of the lower part of the uterus alone. It is not rarely observed after delivery. I have depicted it diagrammatically in Fig. 36. It is, in ordinary circumstances, quite trivial, demanding no treatment, because it soon spontaneously disappears. It is produced under the presence of Radford's conditions for real uterine inversion, or inversion of the fundus, when there is bearing down or any other pressure on the contracted fundus, pushing the uterus downwards through the brim of the pelvis.

Tyler Smith believes that, in the production of inversion, action of the fundus carries or pushes this part into the uterine cavity, where the introcedent part is seized by an hour-glass contraction, and propelled through the lower part of the uterus, then in a state of inertia, but soon to be in a state of active contraction. This theory differs but inconsiderably from that of Radford, as Dr. Smith's own words will show. Nevertheless, Dr. Smith's account of the production of a spontaneous active uterine inversion is the best which I have anywhere met with. "I have always," he says, "been of opinion that, in the great majority of cases, the accident happens in consequence of irregular but active contractions of the uterus itself. No doubt, cases may occur in which inversion is produced by great tension upon the cord, while the placenta is firmly adherent to the fundus, and that all violent traction of the cord, while the placenta is attached to the fundus, is reprehensible. But when

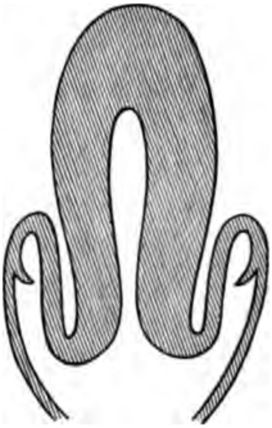

Fig. 36.

Fig. 36. Diagram of inversion of lower part of uterus.

inversion is thus produced, there must be a consenting action of the uterus." . . . "All the facts connected with this catastrophe show that it generally depends, not upon a passive, but an active, condition of the organ." . . . "It has even been known to occur in the unimpregnated uterus". . . . "The nulliparous organ has been known to invert itself, as the result of spasmodic action, in long-continued menorrhagia, or as the result of a small polypus or fibrous tumour in the cavity of the uterus, or upon its peritoneal surface." . . . "There is, first, cuplike depression of the fundus uteri; coincident with, or immediately following upon, this depression, there is hour-glass contraction of the body or lower portion of the uterus. The annular contraction of the body of the uterus grasps the introcedent fundus as it would a foreign body, and carries it downward for expulsion through the os uteri, the os itself being at this time either in a state of inertia, or actively dilated, just as at the end of the second stage of labour. After the inverted uterus has passed through the dilated os uteri, this part of the organ becomes contracted, preventing re-inversion from taking place." . . . "In simple hour-glass contraction, the cavity of the uterus is divided into two parts by the contraction of the middle portion of the organ; but when, owing to irregular action of the fundus, this part of the organ descends into the cavity of the uterus, and the hour-glass contraction then occurs, the fundus uteri is seized by the contracting ring of the uterus, borne down through the os uteri and vagina, and inversion is thus rendered complete." *

Before advancing farther, it will be advantageous, with a view to making my own views more clear, to point out some errors in Tyler Smith's explanations. And, first, I draw attention to his assertion that the unimpregnated uterus, not having a polypus or fibrous tumour in its interior, has been known spontaneously to invert itself. I have no doubt Tyler Smith has considered the recorded cases of the kind, to which he can appeal in proof of his statement, but he has not given any reference to them.† In the meantime I

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* *Manual of Obstetrics*, pp. 480 and 489.

† On this point see some apposite remarks by Dr. West, *Diseases of Women*, third edition, p. 223. "Inversion of the uterus," says he, "the turning of the organ inside out, is an accident clearly impossible in the natural condition of the unimpregnated woman ; it being obviously essential for its occurrence that the organ should have attained a certain size, and that its walls should be comparatively yielding. It is indeed only at an advanced period of pregnancy that these conditions are generally met with, and only during labour that an exciting cause is likely to be superadded capable of producing the misplacement ; but at that time violent traction at the funis by some unskilled practitioner, before the detachment of the placenta, may mechanically invert the womb, or the organ may, by its own contractions, invert itself, just as the intestine does in cases of intussusception. The late Mr. Crosse of Norwich, in his very elaborate essay on *Inversion of the Uterus*, which unhappily he did not live to complete, states (Part ii. p. 70) that in 350 out of 400 cases of inverted uterus of which he had found mention, the accident occurred as a consequence of parturition ; and there can, I think, be no doubt but that the real proportion of cases in which it is traceable to this cause is much higher than 7 to 1. Of the remaining 50 cases, 40 were said to have occurred in connection with the presence of a polypus in the interior of the womb, the accident sometimes taking place spontaneously, in other instances resulting from traction at the out-growth in some attempt to accomplish its removal.

shall only say that I cannot believe that such an inversion ever occurred. The unimpregnated uterus is so thick and hard, and its cavity so small, as to render inversion necessarily slow in its progress, even when a polypus attached to its fundus renders the occurrence of inversion quite intelligible. The polypus, in such a case, is a uterine content, which the uterine walls can seize and expel from the uterine cavity, even though it drags the fundus uteri after it. This slow inversion is an operation, the study of which might have suggested the recently-demonstrated curability of the most chronic inversions. For, if the hard and small-cavitied unimpregnated uterus can be inverted, it is reasonable to expect that the similar but displaced parts of a chronic inversion can be replaced

"Almost all of those rare cases in which the uterus is alleged to have become inverted, independently of either of the above causes, are deficient in such details as are needed to substantiate their correctness, and doubt may be reasonably entertained, with reference either to the accuracy of the diagnosis, or else as to the truthfulness of the history related by the patient. (Baudelocque's remarkable case of alleged inversion of the womb in a girl fifteen years old, who suffered from menorrhagia, appears to me to be one in which we may be allowed to entertain some doubt as to the accuracy of the diagnosis; while nothing can be vaguer than the history of Lisfranc's patient (*Clinique Chirurgicale*, vol. iii. p. 380), whose symptoms are said to have existed five years before she came under his observation.) Enlargement of the uterine cavity, however, associated with some cause capable of exciting contraction of its fibres, may be looked on as the two conditions essential to the inversion of the organ; and when these two co-exist, as in Dr. Thatcher's case of enlargement of the womb from hydatids (as narrated in Crosse's Essay, part i. p. 57), there the possibility of inversion taking place must be conceded." (See also Colombat, *Maladies des Femmes*, tome i. p. 321.)

by means like to those which produced the displacement. The power, which Dr. Tyler Smith and others have shown that we possess, of replacing a chronic inversion, is only not a very great and renowned discovery because of the rarity of the affection to be cured. Similar great results are achieved in various departments of surgery and midwifery by like means—namely, the substitution of gentle and continued effort for more powerful and less continued effort.

But although spontaneous active inversion of the unimpregnated uterus containing a polypus is comprehensible, and although it may be admitted that spontaneous passive inversion of the healthy unimpregnated uterus is conceivable; yet, to me at least, spontaneous active inversion of the unimpregnated uterus, having no organic disease, is quite inconceivable. For it is necessary to suppose that muscular action of its walls could push some part of them into the uterine cavity, should bend some part inwards, which is impossible. Muscular action of a bulging part may make the line of its fibres straight, may destroy the outward bulging curvature of that part of the uterine wall, but can not go further and produce a bulging inward. As a bulging inwards, or the first step to inversion, cannot be attained, spontaneous active inversion is impossible. Holding such views, I cannot accept even the evidence of cases, unless of the most unimpeachable character in every respect.

For the same reasons as are resorted to in the last paragraph, spontaneous active uterine inversion after delivery is impossible, if we adopt Tyler Smith's theory; for no action, regular or irregular, can produce introcession of a part of the uterine wall. Before active uterine inversion can be begun, such introcession must take place. Some part of the uterus must be in a position to be seized by the remainder, must become, in a sense, a uterine content, acting just as a polypus does in cases of inversion in connection with this disease. Some other condition than uterine action must therefore be called in.

Four kinds of uterine inversion occur after delivery :—

1. Spontaneous passive uterine inversion.
2. Artificial passive uterine inversion.
3. Spontaneous active uterine inversion.
4. Artificial active uterine inversion.

The only uterine condition essential to the production of all these kinds is paralysis or inertia, or complete inaction. This is the condition of the whole organ at the time of production of the first two kinds. In the two last kinds it is accompanied by uterine activity, and as these cannot co-exist in the same part, the paralysis is partial, and the activity partial. Action of the uterine wall cannot cause introcession of it. Activity of the whole of the uterus renders inversion impossible. Activity of a part of the uterus renders introcession of that part impossible. There must, therefore, be paralysis of the whole or of a part, before inversion can be begun.

Supposing paralysis, then, how is introcession, or the first stage of inversion, produced? In cases of artificial inversion, cases of the second and fourth kinds, foreign force is applied either from above, to push the paralysed uterine wall into the uterine cavity, or from below, to pull it into the cavity. Spontaneous uterine inversion admits of no foreign power being called in, and we must find the desiderated power in the uterus or in the mechanical conditions of the abdomen. We have already shown that the uterus cannot effect it, and must fall back upon the mechanical conditions of the abdomen. Conditions fitted to produce the result are to be found in the ordinary bearing-down effort, or in absence of the retentive power of the cavity,* however produced.

In cases of active uterine inversion, cases of the third and fourth kinds, it is necessary that the activity or muscular contraction of the uterus be in a part situated below the paralysed part. For as inversion consists in causing the descent of the fundus of the uterus, this cannot be effected by any uterine efforts, except those made by parts below the fundus.

Paralysis or inertia of the whole uterus is a well-known condition. If this condition is required to explain any kind of inversion, it is at least not necessary to enter upon proof of its occurrence.

Paralysis of a part of the uterus is also well known, for every practitioner must often have felt the lower part of the uterus in this state (already described, p. 382) while the upper parts were firmly contracted. But, for our purpose, it is necessary to show that paralysis or complete inertia of the higher parts of the uterus may occur while the lower parts are in a state of active contraction. Of this occurrence, hour-glass contraction is the most frequent exemplification.

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* See *Edinburgh Medical Journal*, December 1865, and the next part of this volume.

Much discussion has taken place as to what is and what is not real hour-glass contraction. By some the term is applied to contraction of the mass of circular fibres at or near the situation of the internal os of the cervix, while others apply the term to such contractions, higher in the uterine walls, as encyst the placenta in the region adjoining the internal orifice of a Fallopian tube. Most British authors use the term without implying any definite locality of the contraction, provided it is not at the os uteri. My experience leads me to join Helié,* and many other authorities, in asserting the occasional occurrence of the contraction in any of the situations already named.

Hour-glass contraction cannot exist unless the parts above the contraction are in a state of inertia, unless there is a local paralysis of the uterus. This paralysis must be perfect, or nearly so, if the hour-glass contraction persists; for were the higher parts of the uterus even in moderate action, the hour-glass contraction would soon be overcome.

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* *Recherches sur la Disposition des Fibres musculaires de l'Uterus*, etc. Paris, 1864, p. 58, etc. etc.

Local paralysis of the higher parts of the uterus generally includes within the affected area that part to which the placenta is attached, or affects that part alone. Just as the placenta is generally found attached to the inverted uterus, so is the placenta almost invariably found in the sac above an hour-glass contraction; and the inactivity of the placental insertion is in both cases frequently shown by the persistency of the attachment. In my own experience I have met with exceptions to these conditions, but they

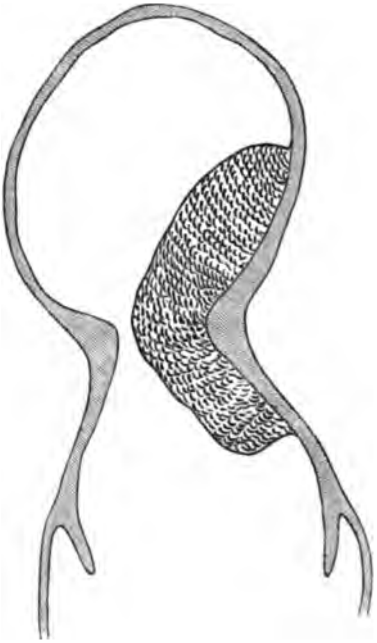

Fig. 37.

Fig. 37. Diagram of the hour-glass contraction in the case recorded.

are universally admitted to form the rule.* As an example of an exception, I may mention that I have met with a persistent hour-glass contraction of only a narrow uterine band running across the insertion of a morbidly adherent placenta. I was called to this case by Dr. Linton.†

In cases of paralysis of the placental insertion, bleeding will naturally be expected to occur if the placenta be partially or wholly separated. But in the case of hour-glass contraction, the bleeding may be prevented by the contraction; and, as Tyler Smith has said, the same prevention of hæmorrhage may arise, in a case of inversion, from "the os uteri acting as a tourniquet to the uterus," and it is more than probable that this condition soon arrives even in cases of passive inversion.

It is worth while here to introduce the observation that the placental site is believed to differ, not only in liability to paralysis but in other more intimate conditions, from the rest of the organ. "Let us remark besides," say MM. Dubois and Pajot,‡ "a fact to which we shall return: it is, that the fibres of the portion of the uterus where the placenta is found inserted, increase less in volume than the rest, and undergo more considerable modifications of their structure. . . . In the internal half of the thickness of the uterine walls, then, occur modifications of structure more considerable. They commence nearly about the end of the second month of pregnancy, and are especially well marked in the site and in the neighbourhood of the placental insertion. These changes consist in the production of molecular, greyish, azotised granulations, soluble in acetic acid, and of some fatty granulations. These last go on increasing in quantity and volume with the periods of pregnancy, and particularly so in the layer immediately contiguous with the caduca."

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* Radford (see the quotation, p. 380) and others erroneously make the rule invariable.

† This interesting case is in exact opposition to the generally true statement of Hegar (p. 39—"Bei totaler Adhäsion ist der Kuchen vollständig in der Höhle enthalten." For the placenta was everywhere morbidly adherent.

‡ *Traité complet de l'Art des Accouch.* p. 419.

In their descriptions of and reasonings upon cases of hour-glass contraction, of inversion, and of hæmorrhage, many authors have erred in taking a hard part of the uterus for a part necessarily in a state of action. Now, while hardness of uterine wall is in most cases a good and sufficient sign of muscular action in the hardened part, it is not invariably so. For, in cases of hour-glass contraction and of inversion, the hardness may arise from passive tension. The bladder of urine and ovarian cysts frequently illustrate hardness of flaccid bags produced by passive distension. In like manner, a paralysed portion of uterus may be hard from being tightly replete with placenta or other contents retained by stricture; and the fundus of an inverted uterus may be hardened by the circumstances of its inversion, apart from muscular action of the hard part.*

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* For some remarks bearing on this point, see Ferguson's Introduction (p. xli.) to the Sydenham edition of Gooch's *Most Important Diseases of Women.*

In the medical literature of post-partum hæmorrhage, especially of long-continued draining, much interesting evidence of the not very rare occurrence of persisting paralysis of the placental insertion is gradually accumulating.* In his work on pathological anatomy, Rokitansky describes it as a disease of the puerperal state as follows :—"We must," says he, "here mention a very singular circumstance, which may, on account of the consequent danger, become important, and may even be misunderstood in post-mortem examinations; it is paralysis of the placental portion of the uterus, occurring at the same time that the surrounding parts go through the ordinary processes of reduction. It induces a very peculiar appearance. The part which gave attachment to the placenta is forced into the cavity of the uterus by the contraction of the surrounding tissue, so as to project in the shape of a conical tumour, and a slight indentation is noticed at the corresponding point of the external uterine surface. The close resemblance of the paralysed segment of the uterus to a fibrous polypus, may easily induce a mistake in the diagnosis, and nothing but a minute examination of the tissue can solve the question. The affection always causes hæmorrhage, which lasts for several weeks after childbirth, and proves fatal by the consequent exhaustion. We have met with it twice, once after abortion, and once after parturition at the full period." . . "Dr. Betschler, during his visit to Vienna in 1840, communicated a similar case to me as having occurred at Breslau; and there can be little doubt that Dr. Burckhardt (vide *Berliner Centralzeitung*, x. 19) speaks of this condition under the title of acute fungus hæmatodes uteri, as of a new and hitherto unknown cause of flooding after childbirth."*

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* In the works of an author so old as Mauriceau we find a case of this kind described by him, and based on a post-mortem examination. I quote from Desormeaux and P. Dubois (*Dict. de Médecine*, tome xxx. p. 349),—"Il y a simple dépression, quand le fond de la matrice est un peu déprimé en dedans, comme est le cul d'une fiole de verre, ainsi que le dit Mauriceau, qui a constaté ce déplacement sur le cadavre." See also Hohl, *Lehrbuch der Gebürtshulfe*, S. 864, for some remarks on this point.

Referring to Rokitansky's description, we find Kiwisch saying,—"Atony of the uterus is either general, or it affects only a part of this organ; and in the latter condition, the partial paralysis of the placental portion, with sinking inwards of it, appears as a peculiar, and, at the same time, very dangerous condition. We have as yet had opportunity to observe it in only a single case."†

Klob also gives an account of the affection—"Defective contraction," says he, "of that part of the uterine wall which forms the placental insertion, is of extraordinary importance. In such cases, this part of the uterus, having the shape of the uvula, or of a body of a conical or round form, with a neck-like constriction, sinks inwards into the distended uterine cavity, while the other parts of the organ seem tolerably well contracted. At the corresponding part of the external surface of the uterus can be seen a funnel-shaped depression, or at least a more or less considerable inversion of the wall. The inverted tumour-like part filling up the uterine cavity is spongy, bleeding, shows the peculiarities of the placental insertion, and in most cases there are on it some remains of the placenta, or there adhere to it coagula of blood in pretty large lumps.

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* *Sydenham Translation*, vol. ii. p. 304. The physical diagnosis of this condition of the uterus may be effected even many days after delivery, should Schrœder's statements be verified. See his *Schwangerschaft, Geburt und Wochenbett*. S. 192.

† *Klinische Vorträge. IVte Auflage*, i. Bd. S. 427.

"Rokitansky points out, that in the origination of such depressions of the paralysed placental insertion, the tuggings, on artificially separating the placenta, might have influence; and I would have this specially attended to, on this account, that I particularly cannot understand wherefore the paralysed portion of the uterine wall always sinks inwards into the uterus, while at the same time, in consequence of the energetic contraction of the other parts of the walls of the uterus, its cavity is reduced to small dimensions. I take it to be absurd to propose to regard the pressure of the abdominal viscera as the cause of the sinking inwards of the paralysed part. Absolute or relative shortness of the umbilical cord may, in such cases, come into prominence as a frequent cause of the pulling.

"The consequences of this condition are hæmorrhages, which may at last lead on to a fatal termination.

"The frequency of this occurrence in abortion is striking, and Engel's first case concerns a woman, 34 years of age, who had already gone through seventeen labours successfully, and in her eighteenth pregnancy suffered an abortion in the fourth month. Engel points out the large number of the pregnancies, and their quick succession, as the cause of the paralysis of the placental insertion, and mentions that in the dead body (and how much more in the living) the swelling formed by the projection inwards of this paralysed part might easily be confounded with something else, as a polypus decomposing at its lower end."*

I shall now, in conclusion, give a brief account of the four kinds of uterine inversion that may occur.

Spontaneous passive† inversion occurs in cases of paralysis, or inertia of the whole uterus; the organ being large, its walls lax, and capable of being inverted by little force. Bearing down produces, in general, collapse and compression of the organ; but it may produce inversion if the depressing force is applied under favourable circumstances, and the inversion will be complete if the bearing down is strong and continued. Should the original condition of inertia persist, the neck not contracting around the inverted organ, then replacement will be at least as easily performed as inversion. It is to this category that I am disposed to refer the cases of inversion post mortem which Bœrner and Klaatsch have recorded.

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* *Pathologische Anatomie der weiblichen Sexualorgane*, S. 264.

† A very good practical account of passive inversion is given by Desormeaux and P. Dubois, at p. 350 of the article already referred to. See also Colombat, *Traité complet des Maladies des Femmes*, tome i. p. 316. This kind of inversion has recently been the subject of some interesting papers by Lazzati, Professor of Midwifery in Milan. He has been erroneously described as forestalling the theory of active uterine inversion given in this chapter. See the *Annali Universali di Medicina*, vol. cxciii. 1865; also the *British and Foreign Medico-Chirurgical Review*, Chronicle, October 1867.

Artificial passive uterine inversion demands little description. It is the kind of inversion commonly described by the older authors. It differs from the spontaneous passive inversion only in this, that foreign force replaces the bearing down. The foreign force may be applied from above, by pushing, or from below, by pulling the cord, or manœuvring with the placenta. It would be a more frequent occurrence than it is, were it not the case that the interference which tends to produce it also tends to bring on that general uterine action which prevents it. It is in all respects similar to the former kind. In both kinds of passive inversion hæmorrhage will probably occur, if the placenta is separated, and the conditions of their production persist.

Spontaneous active uterine inversion is the kind which modern authorship is bringing more and more into notice as the most common kind. I have already said that I am disposed to think this tendency is being pushed too far. In this kind, paralysis of the fundus, or of a portion of it, probably of the placental portion, occurs. The state of the retentive power of the abdomen, or positive bearing down, leads to this portion projecting into the uterine cavity. It is seized by the adjacent contracting segments of the uterus, is pushed down and expelled through the os uteri into the vagina, or beyond the vagina. It is difficult of replacement, in consequence of the contraction of the uterus around the inverted parts.

Artificial active uterine inversion differs in nothing from the kind last described, except in this, that the inversion of the paralysed portion is effected by pressure from above, or by pulling on the cord, or other interference from below.

It can scarcely be considered out of place to add, in conclusion, a few remarks on spontaneous replacement of an inverted uterus. It is almost useless to say that spontaneous replacement of the first stage of an inversion often occurs, and is accounted for by contraction of the inverted part, or by alteration of the condition of the retentive power of the abdomen changing an inversion into an elevation of the paralysed portion of uterus. But spontaneous replacements of completely inverted uteri have been reported by Dailliez* and others. The possibility of this spontaneous replacement has also been denied. But I see no reason to doubt it. An inverted or opposite condition of the mechanism which produces spontaneous passive inversion may also produce spontaneous passive replacement, whether at an early or at a late period. Spontaneous active uterine replacement is inconceivable, because the necessary conditions, being described generally, the same as those for spontaneous active inversion, cannot be supposed to exist.

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* *Dict. de Médecine*, tome xxx. p. 359.

CHAPTER VIII.

CASES OF INJURY TO BONES AND JOINTS IN PARTURITION.

THE following cases are, I think, worthy of being recorded, on account of their rarity and interest:—

Injury of the Sacro-vertebral Articulation.

Mrs. L., æt. 25, had been about three days in labour with her first child, when Dr. Paterson of Leith was called to see her. He ascertained that the child's head, which was unusually large, hard, and ossified, had been impacted in the brim of the pelvis for at least twenty-four hours, and that the pelvis was small. He found the pulse quick, and a fetid discharge flowing from the vagina. A small quantity of thick fetid bloody urine was taken from the bladder. The long forceps were used in vain, but not with much persistence, as the child was dead. Craniotomy was then performed, and a very large fœtus brought into the world with considerable difficulty.

On the patient coming under my care in the Royal Infirmary, exactly five weeks after delivery, the following conditions were observed on exposing the parts and examining them, while she was resting on her back. From the middle of the labia majora, downwards to where the hips touched the table, was an ulcerated excavation, 1½ inches broad, and 3 inches long from before backwards, its deepest part filled with sloughs, among which the urine trickled. The vagina was found crammed with partially-separated dirty yellow sloughs.

On the third day of her residence in the ward, and the thirty-seventh after her delivery, she died of pulmonary embolism, under the following circumstances :—She had in the afternoon been cheerful, and conversing with her husband. At 6 P.M. she took porridge for supper, as usual. About twenty minutes thereafter, the nurse observed a blueness of the face. Soon after, the patient said she felt "faintish," and as if she had taken too much supper. Her uneasiness was confined to the region of the stomach. At half-past six, Dr. Brunton was called to see her. He found her insensible and shivering, so as to simulate the movements of an epileptic fit. The pulse was small, the breathing laboured, the face pale, the lips bloodless. She could not swallow. The pulse soon became imperceptible, the respirations gasping ; and after this state had lasted for about two minutes, death occurred. Dr. Brunton believes he was only about five minutes at the bedside before death.

The post-mortem examination was made by Dr. Grainger Stewart, on the day after death. The following is derived chiefly from his report of it :—

Heart healthy. On the right pleura there is some lymph effused over portions of the lung, into which hæmorrhage (pulmonary apoplexy) had taken place. Both pulmonary arteries contain large partially decolorised clots. Near the brim of the pelvis, considerable serous effusion into the cellular tissue; most marked about the right common iliac vein, which is obstructed by a partially decolorised clot. The clot extends upwards as far as the vena cava, into which its end projects. The other iliac vein, and its branches, natural. Much serous effusion in the pelvic cellular tissue; no pus. Bladder has the appearances of inflammation; there is a wide vesico-vaginal fistula. On the upper and anterior surface of bladder, on both sides, are sloughed openings communicating with the cellular tissue of the pelvis, each somewhat smaller than a florin. The left ureter is partially obstructed, and on the same side there is hydronephrosis. The promontory of the sacrum anteriorly, and the adjoining fibro-cartilage, are roughened and denuded to the extent of nearly the area of a sixpence. At this part, a lamina of the upper articular surface of the first bone of the sacrum is separated from the rest of the bone, and adherent to the fibro-cartilage. The peritoneum over this part is bluish and discoloured, but entire, and without adhesions. Pelvis has a normal appearance, but its conjugate diameter measures only 3⅝ inches; the bis-iliac, 4½.

This case is remarkable on various grounds. The death by embolism is characteristically exemplified; only I wish that the whole symptoms from their commencement had been carefully observed by a physician, tained that the forceps were unskilfully applied by a beginner.

Uterus high in pelvis, and pushed backwards towards sacrum. In front of uterus the pelvis is occupied by an elastic hardness. Urethra tender, swollen, entire. Vesico-vaginal septum entire. Along the left side of the whole extent of the urethra is a deep incised-like fissure, ending anteriorly in a cavity, into which the finger can be only partly passed, behind the vestibulum and clitoris. No bare bone can be felt. From the fissure, a probe can be passed three inches behind the symphysis pubis. The symphysis can be felt and heard to move. In a few days after this examination, the finger could be passed into the pubic joint, between the rough denuded pubic bones. Much pus is discharged from the wounds, and in the urine.

It is not necessary to give a detailed history of the progress of this case. The bladder soon recovered its natural functions. The inter-articular cartilages and some small pieces of bone were discharged soon after she came under my care. The wound slowly healed up. It was not till this took place, about three months after delivery, that the woman was able to attempt to regain her walking power—which she slowly effected.

In this case, I had no doubt that the injury inflicted by the forceps on the tissues to the left of the urethra, and on the lower border of the symphysis pubis, was the cause of all the woman's subsequent misfortunes.

Injury of Right Frontal (?) Bone of Child.

Mrs. C. was delivered by the long forceps of a large child. Traction had been intermittingly used for nearly an hour. The brim of the pelvis was contracted. The mother was well and going about in a few days after delivery. The child also did well. The case was attended by myself and one of my pupils.

I was called to see the child on the fourteenth day of its life, and found a round excavated sore above and in front of the right ear, just where the point of the blade of the forceps had impinged on the skull. From this part there had been discharged a flat round sequestrum of bone, about the size of the flat surface of a split pea.

Ed. Martin, describing the effects of severe and protracted labour in cases of contracted brim, has the following interesting and relevant remarks :—"Simultaneously," says he, "the scalp of the child will be stamped by the easily understood reacting pressure from the side of the projecting promontory. And we find over the corresponding parietal bone of the born child a roundish spot on the scalp, about the size of a groschen, bluish-red and leather-like from compression, which in the higher degrees of compression may be, in the following days, separated by suppuration, so that, as I have seen in one case, the parietal bone itself was laid bare for some weeks and the wound was only slowly cicatrised." In another place, describing a child born in a case of pelvic contraction, he states that on its left parietal bone, which lay in front of the promontory, there was, nearly in the region of the tuber parietale, a mummified piece of skin of the size of a groschen. This almost circular necrosed piece of skin, with the superficial layer of bone of the left os bregmatis, was separated by suppuration, so that, four weeks afterwards, white bone could be seen at the bottom of the ulcer. Some time afterwards the ulcer healed, the cicatrix of the skin adhering intimately to the subjacent bone.*

Dr. Priestley has related an interesting case of sloughing of the fœtal scalp after a long labour, and he refers to two coloured drawings of fœtal heads that had suffered similar injuries, in the possession of Dr. M'Clintock.†

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* *Die Neigungen und Beugungen der Gebärmutter.* Berlin, 1866. S. 133.

† *Trans. Obst. Soc. of London.* Vol. i. 1860. P. 323.

PART V.

MISCELLANEOUS.

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CHAPTER I.

ON THE RETENTIVE POWER OF THE ABDOMEN.

So far as I know, the abdominal cavity has hitherto been regarded as possessing no other mechanical qualities than those of a closed box or bag, with walls of various and variable degrees of mobility, firmness, and softness; and the various viscera have been held as being maintained *in situ* under every condition by their ligaments and by their apposition or packing within this cavity or bag. The object of this chapter is to show that, while all this is true, it does not express the whole truth; that the abdomen is not a mere bag packed full; that in health or in ordinary circumstances it has another mechanical quality which has important physiological bearings; that this additional mechanical quality may be present in an extraordinary degree, and that it may be diminished below the ordinary degree or entirely absent, and that these states of increase or lessening of this quality have important physiological and pathological bearings.

This quality I name "retentive power," because one of its chief functions is to aid in maintaining the viscera in their natural position in the cavity.

The same mechanical quality is possessed, in what may be almost called an infinitely higher degree, by the thorax; but there it does not merit the same name, as its functions in connection with the respiration and circulation are paramount, and reduce to insignificance any mere retentive quality which gives the name to its abdominal analogue. This higher degree of analogous power in the thorax depends on the greater solidity and methodised mobility of its walls, and on its more complete closure or more complete potential closure.

The preceding paragraphs give my opinions when I first published my paper on this subject in the *Edinburgh Medical Journal.* But, besides some casual indications of belief in the retentive power of the abdomen to which I shall in the course of this chapter refer, I have found in John Bell's *Discourses on the Nature and Cure of Wounds,* to which I was referred by Professor Turner, statements distinctly indicating that this acute surgeon knew that the abdomen was more than a mere bag packed full of viscera.

"There is not, truly," says he, "any cavity in the human body, but all the hollow bowels are filled with their contents,—all the cavities filled with their hollow bowels, and the whole is equally and fairly pressed. Thus, in the abdomen, all the viscera are moved by the diaphragm and the abdominal muscles, upwards and downwards, with an equable continual pressure which has no interval; and one would be apt to add, the intestines have no repose, being kept thus in continual motion; but though the action of the diaphragm, and the re-action of the abdominal muscles, is alternate, the pressure is continual; the motion which it produces is like that which the bowels have, when we move forwards in walking, having a motion with respect to space, but none with regard to each other, or to the part of the belly which covers them;—the whole mass of the bowels is alternately pressed, to use a coarse illustration, as if betwixt two broad hands, which keep each turn of intestine in its right place, while the whole mass is regularly moved:—When the bowels are forced down by the diaphragm, the abdominal muscles recede: when the bowels are pushed back again, it is the reaction of the abdominal muscles that forces them back and follows them; there is never an instant of interruption of this pressure, never a moment in which the bowels do not press against the peritoneum; nor is there the smallest reason to doubt that the same points in each are continually opposed. We see that the intestines do not move, or, at least, do not need to move, in performing their functions; for, in hernia, where large turns of intestines are cut off by gangrene, the remaining part of the same intestine is closely fixed to the groin, and yet the bowels are easy, and their functions regular. We find the bowels regular when they lie out of the belly in hernia, as when a certain turn of the intestine lies in the scrotum or thigh, or in a hernia of the navel; and where yet they are so absolutely fixed, that the piece of intestine is marked by the straitness of the rings. We find a person, after a wound of the intestine, having free stools for many days; and what is it that prevents the fæces from escaping, but merely this regular and universal pressure? We find a person, on the fourth or fifth day, with fæces coming from the wound! a proof, surely, that the wound of the intestine is still opposite, or nearly opposite, to the external wound. We find the same patient recovering without one bad sign! What better proof than this could we desire, that none of the fæces have exuded into the abdomen?

"If, in a wound of the stomach, the food could get easily out by that wound, the stomach would unload itself that way,—there would be no vomiting, the patient must die; but so regular and continual is this pressure, that the instant a man is wounded in the stomach he vomits, he continues vomiting for many days, while not one particle escapes into the cavity of the abdomen. The outward wound is commonly opposite to that of the stomach, and by that passage some part of the food comes out; but when any accident removes the inward wound of the stomach from the outward wound, the abdominal muscles press upon the stomach, and follow it so closely, that if there be not a mere laceration extremely wide, this pressure closes the hole, keeps the food in, enables the patient to vomit, and not a particle, even of jellies or soups, is ever lost, or goes out into the cavity of the belly.

"How, without this universal and continual pressure, could the viscera be supported? Could its ligaments, as we call them, support the weight of the liver—or what could support the weight of the stomach when filled? Could the mesentery or omentum support the intestines—or, could its own ligaments, as we still name them, support the womb? How, without this uniform pressure, could these viscera fail to give way and burst? How could the circulation of the abdomen go on? How could the liver or spleen, so turgid as they are with blood, fail to burst? Or what possibly could support the loose veins and arteries of the abdomen, since many of them—*e. g.* the splenic vein is two feet in length—is of the diameter of the thumb, and has no other than the common pellucid and delicate coats of the veins? How could the viscera of the abdomen bear shocks and falls if not supported by the universal pressure of surrounding parts? In short, the accident of hernia being forced out by any blow upon the belly, or by any sudden strain, explains to us how perfectly full the abdomen is, and how ill it is able to bear any pressure even from its own muscles, without some point yielding, and some one of its bowels being thrown out. And the sickness and faintness which immediately follow the drawing off of the waters of a dropsy, explain to us what are the consequences of such pressure being, even for a moment, relaxed.

"But, perhaps, one of the strongest proofs is this, that the principle must be acknowledged, in order to explain what happens daily in wounds; for, though in theory we should be inclined to make this distinction, that the hernia or abscess of the intestines will adhere and be safe, but that wounded intestines not having time to adhere, will become flaccid, as we see them do in dissections, and so falling away from the external wound, will pour out their fæces into the abdomen, and prove fatal; though we should settle this as a fair and good distinction in theory, we find that it will never answer in practice. Soldiers recover daily from the most desperate wounds; and the most likely reasons that we can assign for it are, the fulness of the abdomen, the universal, equable, and gentle pressure; and the active disposition of the peritoneum, ready to inflame with the slightest touch; the wounded intestine is, by the universal pressure, kept close to the external wound, and the peritoneum and the intestine are equally inclined to adhere; in a few hours that adhesion is begun which is to save the patient's life, and the lips of the wounded intestine are glued to the lips of the external wound. Thus is the side of the intestine united to the inner surface of the abdomen; and though the gut casts out its fæces while the wound is open, though it often casts them out more freely while the first inflammation lasts; yet the fæces resume their regular course whenever the wound is disposed to close; or if the fæces should not resume their natural course, we may force them onwards, by closing the wound with a plaster, thus healing the fistula, which otherwise might never close."[*]

Evidence of the existence of the retentive power of the abdomen is obtained by a study of the position of the unimpregnated uterus. This organ is furnished with ligaments which permit its movement in every direction. In its healthy or ordinary condition, these ligaments are not on the stretch.[†] It may be pulled down to a small extent without tightening them; it may be elevated to a small extent without tightening them; and it may likewise be moved in any other direction. Were there no retentive power in the abdomen, the uterus would of course fall down till it received support from the ligaments above or from the subjacent parts. It does not do so, but floats in the pelvis. When by the post-mortem opening of the abdomen—and the same would be true of the living female—the retentive power of the closed cavity is annihilated by admission of air, then in ordinary circumstances the uterus does fall down from its natural position to a position in which it is supported by the ligaments or by the floor of the pelvis. These facts are inexplicable on any theory that I can conceive, unless they are accounted for by supposing a retentive power in the abdominal cavity.

Many other uterine phenomena are in a like manner explained, as the ordinary descent of the organ when its weight is, by any cause, increased, as by early pregnancy and by tumours; as also the ordinary ascent of the uterus when its weight is diminished, as in the atrophy of old age, when it is well known to draw up the vagina into the conical shape of a tent. These phenomena may own either of two immediate causes, namely, increased or diminished weight of the organ, or increased or diminished retentive power of the abdomen; but in both cases it is necessary to suppose that the retentive power exists.

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[*] *Discourses on the Nature and Cure of Wounds*, 3d Edit. 1812. P. 323.

[†] See *Edin. Med. and Surg. Journal* for 1864, p. 327.

Joulin acutely remarks,—"The most remarkable and the most inexplicable phenomenon which the uterus presents in the course of its evolution, is the change of place by which it leaves the excavation of the pelvis and mounts into the abdominal cavity against all the laws of physics, and without any anatomical reason to explain the ascending movement.

"It is surprising that a fact so strange has not attracted the attention of any of the authors who have written on midwifery.

"The uterus in a state of vacuity is found entirely confined in the excavation; under the influence of pregnancy it increases in weight and volume, and nevertheless it rises in spite of the pressure of the intestines, the action of the diaphragm, the resistance of the fibrous and muscular elements of the abdominal wall. To it there is nothing but resistance in this direction. It rises more and more according as its volume increases, as its weight becomes greater—that is, as the resistance becomes more energetic; it rises in spite of the laws of gravity, repels the intestinal mass, the diaphragm, mechanically distends the abdominal wall, and at last is found situated above the superior strait.

"By what forces does it make out a point of support? What is the lever which causes the mass to move? I have in vain interrogated the textures which surround the organ. I have not been able there to find a satisfactory answer to these questions, and I prefer avowing my ignorance on this point to advancing hypotheses destitute of value. Nevertheless it is probable that the horizontal decubitus maintained by the woman during the night favours the action of the unknown force which produces the change of place.

"If the problem be actually insoluble, it appears to me at least interesting to propose it." *

This ascent of the pregnant uterus forms, I believe, an illustration of the physiological or natural use of the retentive power. I do not deny that this phenomenon may sometimes be all effected by the mere gradual increase of bulk of the uterus, favoured by occasional alterations in the pregnant woman's position. But it appears to me that, were this generally the case, uterine intumescence, even though gradual (and whether natural or morbid), would very frequently cause, by pressure, much suffering, which we know not to be the necessary or ordinary companion of the changes. As the uterus enlarges in early pregnancy, sometimes also as it enlarges in disease, the base of

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* *Traité Complet des Accouchements*, p. 331.

the throax of the female is expanded,* the capacity of the abdominal cavity is increased, and the uterus, instead of forcing its way up against the influence of gravitation, is probably actively drawn up, at least to some extent, into its new site. In some cases, as I shall hereafter mention, this drawing up is abnormal or extraordinary in force, and the uterus is then entirely sustained by the drawing-up forces, being removed from solid subjacent support.

The often-observed retrocession of the child's head, in the intervals of the pains during the latter part of the second stage of labour, has been made the subject of various explanations. But I am sure that none of them is, in the main, correct, and that an easy explanation is found in the power here described. When the bearing down, which diminishes the retentive power, ceases with the uterine contraction, the retentivity is rapidly increased and draws the child back again into the previously partially evacuated abdominal cavity.

At this place I may direct attention to the occurrence of emphysema of the pelvic cellular tissue and of the hypogastrium generally; of which M'Clintock and others have given careful and valuable descriptions,* but have offered, so far as I know, no explanation. This remarkable occurrence is believed to be in ordinary cases, a result of purely mechanical causes, and I see no reason to seek any other for its production in the pelvic regions in certain difficult labours. As Dr. M'Clintock suggests, in order to explain some, so I apply to all the remark, that the air obtains entrance to the cellular tissue through some rent in the genital passages. And I believe that the theory of the retrocession of the head, just described, affords a simple and easy illustration of the mechanism of the production of emphysema in obstetric cases.

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* "According," says Grenser (*Lehrbuch der Geburtshülfe*, S. 123), "to the exact measurements of Dohrn, by means of a jointed chain called a cyrtometer, the base of the thorax exhibits in most cases a greater breadth in the last weeks of pregnancy than during the puerperal state; on the other hand, a smaller depth from before backwards. It appears therefore that the thorax during pregnancy gains as much in breadth as it loses in length, as Küchenmeister and Fabius had already discovered by spirometric experiments.—*Monatsschr. f. Geburtsk.* Bd. xxiv. p. 414." But this statement of Grenser's does not appear to me to state nearly the whole truth. It is well known that many women find, as a result of pregnancy in the earliest months, that the girth around the waist is considerably increased.

If we now direct our attention to the uterus after delivery, a solid mass of about two pounds in weight, we find strong circumstantial proof of the action of this force. Delivery being completed, the contracted uterus is left by the accoucheur partly in the cavity of the pelvis, partly projecting above it. It soon loses its intense hardness, changes in shape and increases in bulk, its active contractions no longer going on. Let us suppose the patient to be kept lying on her back: this weighty mass soon no longer rests in its former situation; it has risen out of the pelvis, and occupies a large part of the abdomen below the umbilicus; it may even be higher. It has no corporeal connections with the middle of the abdomen by which it could be elevated thither; its connections are all at its lower extremity, about the roof of the pelvis, and if they act at all, can only do so by drawing it down. It has ascended against the gravitation of its own weight, against the resistance of superincumbent bowels and anterior abdominal flap, all of which would keep it down; perhaps also against the influence of a tight binder. It has ascended almost vertically if the woman be regarded as in the erect position: with the body horizontal, it has ascended in a direction oblique to the horizon. Were there no such force as the retentive power of the belly, it appears to me impossible to conceive an explanation of this phenomenon. If we study the abdomen as possessing a retentive mechanism, the phenomenon is easily explained, and at the same time a solution is acquired of many cases of retention of urine in women newly delivered, who have no paralysis of the bladder, no spasm, and no swelling of the urethra or vulva. In advanced pregnancy, the base of the chest is greatly expanded, and the capacity of the upper abdomen greatly increased: from this resisting or moderately firm thoracic circle descend the walls of the abdomen, to join inferiorly a quite solid ring of pelvic bone. To expel the fœtus, straining efforts have been vigorous: these first annihilate retentive power, and, continuously increasing, gradually produce increasing expulsive force. The uterus is evacuated, the abdomen greatly emptied also.

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* *Dublin Quarterly Journal*, vol. xxiv. 1857, p. 450. See, for reports of other cases, pp. 98, 102, 120, and 132 of Hardy and M'Clintock's *Practical Observations on Midwifery*, etc.

This sudden emptying, while the woman lies, is generally followed by complete annihilation of retentive force, the abdominal walls being perfectly relaxed. The uterus rests in the pelvis, and bleeding is prevented by active uterine contraction. But, as the abdominal walls resume tone, they re-establish a retentive mechanism for the abdomen. The woman breathes easily, expands her chest; the uterus is drawn up into the abdominal from the pelvic cavity; and, simultaneously with the cessation of active uterine contraction, the tendency to bleeding is partly arrested by the retentive action of the belly aiding the natural course of the blood.

Taking it for granted that, contrary to the general rule, the uterus when increased in weight, even when very heavy, does not rest on any underlying support [as it must do were there no retentive power in the abdomen], we have in such an occurrence illustration and proof of the occasional excess of retentive power, or extraordinary amount of it. That clinical experience furnishes and demonstrates such phenomena, I do not here stop to insist; satisfying myself with stating, what I believe all experienced obstetricians will confirm, that cases of early pregnancy and of fibrous tumour of the uterus (and of ovarian dropsy), do rarely occur, in which the organ floats high among the small intestines, taking no sustaining assistance from the brim of the pelvis. That such heavy masses should be elevated into the abdominal cavity, and remain so suspended without any solid support, appears to me insusceptible of being otherwise accounted for than on the theory here advanced.

That the study of this power of the abdomen will throw light on many cases of displacement of the uterus, especially of retroversion of the gravid uterus, I have a strong conviction. For without calling it in, without a diminution or deprivation of the power, I cannot comprehend the occurrence of retroversion of the gravid uterus in some cases, its difficulty of replacement in others, and its recurrence after replacement in a third class of cases.

The existence of this power, not as maintaining a condition of the abdominal organs, but dynamically effecting changes, is illustrated by the following phenomena. After straining efforts to empty the bladder, a catheter being in the urethra, the evacuation of the urine is occasionally followed by the inward rush of air into the bladder when the straining ceases. The same is noticed sometimes when, instead of straining, there is pressure exerted over the hypogastrium followed by its removal. The presence of air in the bladder I have observed also under other circumstances, apart from catheterism or straining, and dependent on the position of the body on the side, while the anterior abdominal wall is relaxed. Everybody knows what will happen if, before an inspiratory effort, an opening is made in the walls of the chest. The rushing of air, above mentioned, into the bladder is an analogous phenomenon, and the familiar occurrence of the same kind at the end of a tapping in a case, say of a unilocular ovarian cyst, is still another. In the cases where I have observed this to happen, it has been easily explicable. During the evacuation of the last of the cystic contents, the anterior wall of the abdomen, and specially the recti muscles, are, by the hand or a bandage, pressed backwards towards the spine, and come to form a crescentic concavity, whose extremities are the anterior part of the base of the thorax and the pubic bones. Contraction of the recti muscles tends to destroy this concavity, to increase the capacity of the abdominal cavity above what is, by other alterations on it, compensated for, and to produce the rushing in of air to fill the partial void, just as air rushes into the wounded chest on its expansion in the act of inspiration.

I have no doubt that the expulsion of air from the genital passages of women, so often described, is susceptible, in most cases, of easy explanation, by the aid of this power. In some circumstances air is drawn into the passages, to be again, under different circumstances, expelled. This accords with the views regarding uterine tympanites expressed by Dr. Keiller and others.

In connection with the physiological uses of the retentive power of the abdomen, I think the maintenance of the position of the whole viscera deserves careful study. The case of the weighty liver, situated in its upper regions, is well adapted for this purpose, but its apposition to the diaphragm superiorly introduces yet an additional element into the causes of its maintenance of its position. At first sight, the ligaments appear to be unequal to the task they are generally supposed to have to perform. Alone they appear very slender, and to consist of structures which are not adapted to support for long periods of time, unyielding, such a weighty lump as the liver. Moreover, they have no attachments at either extremity such as would seem to indicate their function. And if so ill adapted for the continued support of the liver when the body is at rest, how much less are they able to resist the sudden impulses communicated to the organ in running and jumping—impulses which, given to so consistent and heavy a mass, must have great force—impulses which are not resisted by any solid subjacent structure, but by the mass of air-containing, and therefore elastic, bowels! In cases of great violence, such as certain falls, the momentum of the liver, when the body is arrested in its course, is so great as to detach it from its diaphragmatic connections and position, and lodge it in the pelvis, in spite of resistance of every kind. When the abdomen is laid open, and air admitted above the liver, between it and the diaphragm, so as to leave it to the support of its unaided ligaments, and if then the trunk be held vertically and jerked with violence downwards, the insertion of the falciform ligament into the upper surface of the organ is readily detached, not by laceration of the ligament, but by its elevation from the subjacent liver-tissue. Were there no means of support for the organ during life but the ligaments and adjacent elastic viscera, the like effect would surely result from similar violence applied in jumping and the like. Without being able to adduce positive proof that, besides the ligaments and the adjacent viscera, the liver has other means of support, I have no hesitation in asserting my belief that it has, generally, some force contributing to the maintenance of its position from the retentive power of the abdomen. It is attached to the inferior surface of the diaphragm by at least the pressure of an atmosphere, there being no intermediate elastic fluid such as air, and it being supposed that there is no intermediate layer of watery fluid: besides, it has for its support whatever above this atmospheric pressure may be the special retentive power of the abdominal cavity. Without these adjuvants, I cannot imagine the residence of the organ in its locality, in the erect position of the living man, without producing discomfort, and, in certain frequently-occurring circumstances, danger. The peculiarity of the liver's position immediately beneath the diaphragm secures for it always, if nothing intervenes between it and the diaphragm, the support of the pressure of an atmosphere, whatever may be the state of the retentive power of the particular abdomen in which it is. This peculiarity may render special security to the liver and spleen, apart from the general retentive power of the abdomen exerted on them and on the other viscera as well. If there be any truth in these views, the ligaments of the liver and the diaphragm may assist in preventing displacement of the viscus; but they never do, in any sense, bear the weight of it: it is supported by aërostatic pressure more than equivalent to the office.

The statement just made, that the liver is attached to the inferior surface of the diaphragm by at least the pressure of an atmosphere, is truly only if no fluid lie between the diaphragm and liver. If a layer of watery fluid exist between the upper surface of the liver and the diaphragm, then the atmospheric pressure will be diminished between the diaphragm and liver only by a small amount depending on capillarity, and this small amount will give a little support to the liver, but to an extent quite insignificant. In more familiar terms, this presence of a layer of watery fluid is very nearly equivalent to a layer of air between the liver and diaphragm. If, then, contrary to the assumption in last paragraph, a layer of watery fluid exists between the liver and diaphragm, the weighty viscus depends, for the maintenance of its position, almost entirely on the retentive power of the abdomen, in addition to the forces generally recognised and named.*

It can scarcely be considered out of place to direct attention to a physiological function of this retentive power, although it does not come directly within the scope of these notes, and it be one to which the name "*retentive power*" is decidedly inappropriate. Physiologists have, I believe, often felt the difficulty of accounting for the ascent of the column of blood from the lower limbs to the venous opening in the diaphragm. The *vis a tergo*, the influence of contractions of neighbouring muscles, and the action of the valves, seem all, taken together, inadequate to the effect; and this inadequacy is the more striking if attention is directed to the great column of blood in the vena cava abdominalis in a man in the erect position.*

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* For the suggestion of this paragraph, and for other criticism, I am indebted to Professor Tait.

If the abdomen be admitted to have the retentive power here claimed for it, then two great forces are brought into play, which will aid the ascent of fluids, of blood, and of chyle, into the thorax. For if the abdomen be supposed to be merely a closed, and practically an unyielding box, without any positive or active retentive power, the actions of the heart and chest in inspiration will attract the abdominal blood, not merely in the vicinity of the diaphragm, as it attracts that in the jugular veins, but along the whole course of the vena cava, and even as far as the utmost limit of the abdomen. The soft and apparently easily depressible condition of the anterior abdominal wall may appear sufficient to destroy all such remote influence of the thoracic actions; but this cannot, of course, be the case if the retentive power is held to be proved. Besides, the analogy of the brain seems to afford conclusive evidence in favour of the possibility of such a transmission of influence to a great distance. The denuded brain is known to exhibit the respiratory pulse, changes dependent on the same thoracic actions and transmitted through the blood-vessels of the neck, which do not present any mechanical advantage for this object over the abdominal vessels. Further, in addition to this influence, or force, which will operate if the abdomen has any retentive power, the ascent of the blood into the abdomen, and through it, will be favoured in proportion to the degree of the retentive power. The first force acts if there be any retentive power; the second is an addition to the first, and is in direct proportion to the amount of the retentive power. As we believe in the existence of this retentive power, so we believe that both these forces are adjuvant to the venous circulation. The congestion of the vessels of the intestines, observed on opening the abdominal cavity, may be dependent on the influence of the loss of these adjuvant forces felt even in the horizontal position. And it may be anticipated that further researches will show a connection between modifications of these forces and various states of the bowels, as well as some abortions—menorrhagia, metrorrhagia, hæmorrhoids, and other like diseases depending, partly at least, on conditions of the vascular system.[*]

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* On this subject see some remarks by Berard. *Cours de Physiologie*, tome iv. p. 72.

Having shown that this so-called retentive power exists and has important functions, it would be proper for me now to enter upon the description of its mechanism, its degree or force, the causes of its variations, and the means which might be employed to increase or diminish it, as medical practitioners might desire. In the meantime, I do not discuss each of these subjects specially, having stated in the course of the considerations submitted in this chapter most of what I know or believe in regard to them. This knowledge is very scanty and deficient. Further inquiries will serve to extend and improve it, or perhaps to afford another explanation of the whole phenomena—a result which will be as advantageous to science and practice as the other alternative. In the rest of this paper I shall add illustrations of the force from various sources, natural and morbid; and it must be remembered that, among the first supporters of a novel theory, one of the most satisfactory consists in the number of old difficulties and incongruities which it gathers around it to get their resolution or reconcilement.

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[*] The following statement, indicating a suspicion of some abdominal influence upon the venous circulation, is by Ludwig (*Lehrbuch der Physiologie der Menschen*, II. Band. S. 147) :—" Die Verkürzung oder Erschlaffung der Bauchmuskeln, wodurch der Inhalt der Unterleibshöhle sehr verschiedene Spannungen erfährt, muss natürlich auch unter-

The power here described appears to me to be ordinarily increased by the erect position, and by exertions made in that position; and assuredly there is need for its increase in these circumstances. The abdominal walls are rendered tense, and the inspiratory effort will therefore have greater adspiratory power in the abdomen. No doubt the descent of the diaphragm appears, at first sight, to produce lessening of the ab-

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stützend oder hemmend auf den Blutstrom wirken, da in der Unterleibshöhle grosse Gefässe eingeschlossen sind. Die Beurtheilung der Verhältnisse bietet keine Schwierigkeit." On the same subject, see some remarks by Berard, who also gives some references—*Cours de Physiologie*, tome iv. p. 72.

dominal cavity, and therefore diminution of retenti power. But this lessening is very doubtful; for whi this muscle descends and diminishes the vertic depth of its own concavity, the attachments of it a elevated and the base of the chest expanded— change which will compensate for the lessening b the so-called descent of the diaphragm, or perhap more than compensate. If, in experiment, one trie to destroy the retentive power of the abdomen, an produce its opposite—that is straining, as at stool— one cannot fail to see what an extended distance there i between the descent of the diaphragm in mere inspiration and its descent with combined action of th muscles of the anterior abdominal wall, as in th mildest straining efforts.

The increased capacity of the abdomen in certai positions leads to dynamical exhibition of its retentiv power, as in drawing air into it. In some attitudes as resting on the knees and elbows, air rushes throug the anus into the rectum in some individuals. I Sims' position for the operation for vaginal fistula air passes into and distends the vagina. I hav repeatedly observed the vagina become filled an distended tightly with air in making examinations o performing minor operations, the woman lying in th ordinary position on her side. The mechanism b which, under these circumstances, air is actively draw into the vagina, I do not attempt to describe. That i is a powerful adspiratory abdominal effort there ca be no doubt; and I have no hesitation in suggestin its probable important interference in various natural and morbid processes. Of these I shall mention some. Its presence during copulation will contribute to, if not produce, the ascent of the semen into and through the uterus and tubes, at least as far as there is a free or available passage.* The relaxation of the os uteri, if not its openness, is a condition which physiologists have often supposed to exist during copulation: if it does exist, then the mechanism we have mentioned explains at once the ascent of the semen to the fundus uteri; and, if the tube is open, even to the ovary. Here it is worth while to allude to the erection of the uterus as described by Rouget. This erection, in an extreme degree, produces, according to this experimenter, changes which may act as a local adjuvant force in two ways, producing rigidity of the uterine tube and separation of its walls; and, in doing so, producing a vacuum in the uterine cavity, which will attract free fluid into it by an injaculation contrastible with male ejaculation. Describing the changes produced by post-mortem injections, Rouget says:— "L'uterus devient plus convexe en avant et en arrière surtout; ses bords, précédemment amincis, s'arrondissent et se developpent, de telle façon, que l'organe, après l'injection, présente un volume de moitié au moins plus considérable qu'à l'etat de vacuité; en même temps les parois de la cavité utérine s'écartent comme Günther et Kobelt l'ont montré pour les parois de l'urètre."*

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* Such cases as are described or referred to by Marion Sims, at p. 371 of his *Clinical Notes on Uterine Surgery* cannot, it appears to me, be explained by any other known power than that described in this paper. Blumenbach, referred to by Mason Good (*Study of Medicine*, 1822, vol. iv. p. 26), seems to have imagined some such power of "sucking in" as I here wish to imply.

The same mechanism may, in certain abnormal conditions, explain the backward flow of blood, or of lochia, or of pus, from the uterus into the peritoneal cavity: it may also explain the abnormal wanderings of the ovum, or its abnormal position in any case of extrauterine pregnancy. If air or lochia may pass into the uterine sinuses, the phenomenon may receive an identical explanation.†

In the natural performance of the functions of defæcation and urination, there appears to me to be nothing of the nature of straining or expulsive abdominal effort. Efforts are made which, if at once increased and continued, would be straining or expulsive. But, in analysing my own actions as far as I can, I detect no expulsive effort, but only such an amount of like exertion as destroys the retentive action of the abdomen. And it appears to me that, in many recently-delivered women, the cause of retention of urine may be found in the above circumstance, that in the recumbent position the abdominal retentive power is so great that these slight straining efforts do not reach even the length of its annihilation in their special condition. And the same explanation may apply to the difficulty of urination and defæcation while lying felt by many men as well as women.

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* *Journal de Physiologie*, tome i. 1858, p. 339.

† I have elsewhere (*Edinburgh Medical Journal*, November 1865. p. 407) made some remarks on this subject. But I must here add the statement of my present belief that the passage of air from without along the uterine sinuses into the venous system, as alleged by Cormack, Simpson, and many others, is impossible; that the evidence for its occurrence is not sufficient to overcome the disinclination to believe in its mechanical possibility. The uterine contractions which are alleged to force the air into the uterine sinuses, will also equally shut the sinuses against its admission. This and many other difficulties it would be easy to raise, did I consider this the proper place for doing so. On this subject see some remarks by Berard, *Cours de Physiologie*, tome iv. p. 97. See also a paper by Henderson, *Edinburgh Medical Journal*, August 1866; also Joulin, *Traité Complet des Accouch.* p. 1199.

This retentive abdominal action I have already mentioned as favouring the natural course of the blood. Its diminution or absence will be, to some extent, a cause of varicose veins of the lower limbs and of hæmorrhoidal congestion. A further stage than mere absence of retentive action, straining, produces engorgement of hæmorrhoids. In disease, it frequently happens that the mere absence of the retentive power produces hæmorrhoidal bleeding, as in easy going to stool. In women suffering from menorrhagia or metrorrhagia, there may be little loss even in the erect position, except when the retentive action of the abdomen is suspended. Such women often lose very little while walking about, but in urination or defæcation bleed copiously.

It is easy to suggest the great influence of this function of the abdomen in such diseases as prolapse of the pelvic viscera, retroversion of the gravid uterus, hernia, versions and flexions of the uterus. In the meantime. I conclude with the hope of returning to the more detailed consideration of these topics, and of the improvements in practice which this study may be the ground of suggesting.

Before concluding, I must point out one evident source of great difficulty in solving the problem of the mechanism of the retentive power—namely, the varying amount of flatulence in the distensible and movable bowels. This variation is not only in different individuals, but in the same individuals at different times. At present we have no estimate of the increased tension of intestinal gases produced by the muscular walls of the bowels, nor have we any estimate of the diminished or altered tension of the whole abdominal elastic fluids produced by its retentive mechanism. In connection with the amount of intestinal gases comes to be considered the facility or rapidity of their secretion or development, and the force of the closure of the intestines against the admission of air from without.*

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* For some remarks on the abdominal cavity I have to refer to the fifth memoir of Maissiat. *Études de Physique Animale.* Paris, [illegible].

CHAPTER II.

ON SOME POINTS IN UTERINE METROLOGY.

THE external superficial area of the unimpregnated uterus has been measured by Levret. In his *Art des Accouchments,*[*] he describes it as having 16 inches, or nearly so, of surface. I am not disposed to enter on any minute criticism of Levret's measurement; only, I may say that it appears to me to be excessive. A length of 3 inches and a breadth of 2 is more than the full superficial dimensions of the uterus externally; and this gives, in a flattened pyriform organ, a surface of about 12 square inches.

The superficial area of the internal surface of the cavities of the unimpregnated uterus has not been measured. Indeed, it is almost impossible to do so. It may be estimated roughly, and perhaps rather in excess, from Guyon's moulds, that the superficial area of the cavities of the uterus is from 1 to 2 square inches, and that of the body alone (exclusive of the cervix) from ½ to 1⅓ square inch.

The capacity of the unimpregnated uterus has been carefully measured by Levret, Krause, Dubois, and Guyon. Levret[†] estimates it at $\frac{11}{14}$ of a cubic inch. Speaking of the capacity of the nulliparous uterus, M. Guyon remarks,*—"We have tried to utilise the solidifying injections for the study of the capacity of the cavity of the uterus; we have, with this view, plunged the moulds alluded to into a graduated test-glass containing pure water. Their capacity, valued as exactly as possible, is from 3 (⅙ cubic inch) to 5 cubic centimètres (⅓ cubic inch). (This last figure applies only to very large cavities.) Thus we come very near the estimate of Professor Dubois, who fixes this capacity of the cavities at 2 (⅐ cubic inch) or 3 cubic centimètres (⅙ cubic inch.) Krause, therefore, has exaggerated this capacity in making it in the virgin from 1½ to 2 cubic inches.† The same exaggerated estimate is again made by this author for the capacity of the cavities of multiparous uteri, which we shall presently consider." In another place, M. Guyon remarks,‡—"The capacity of the cavities of the multiparous uterus has varied, in our experiments, from 5 to 8 cubic centimètres (⅓ to ½ cubic inch). The figure 5 refers to two cavities, whose diameters gave the medium measurement that we have shown; the figures 7 and 8 refer to our other experiments, having thus for their subject large cavities, surpassing the medium size. We are therefore of opinion that the capacity of the cavities of the multiparous uterus should be valued at from 5 to 6 cubic centimètres" (⅓ cubic inch).§

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[*] Edit. 1761, p. 299.

[†] *Loc. cit.*

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* *Journal de la Physiologie.* Paris, 1859, p. 215.

† *Encyclopédie Anat.* t. v. p. 143.

‡ *Ibid.* 402.

§ See also some measurements by Sappey, *Anatomie descriptive.*

It would be a work of over-refinement to measure separately the external and internal superficial areas of the walls of the uterus in advanced pregnancy, because of the great variations in size of different organs, and because of the variations in size even of the same replete organ under different conditions.

The foundation of such measurements is the estimate of the average linear dimensions of the full-sized gravid uterus—that is, of an organ having a more or less perfect ovoid shape. Now, the best authorities have varied considerably in their statements of such average measurements.

Velpeau* has the following:—"It is said that, at the full time, the vertical diameter of the uterus is 12 inches, the antero-posterior 9 inches, and the transverse 8½ inches. In three women, dead at the full time, before the rupture of the membranes, I have found twice 15 inches, and once 13 inches in the great diameter; once 8 inches and twice 10 inches from before backwards; twice 11 and once 9 inches transversely. At the height of the tubes its circumference is about 26 inches, and only 13 inches at the level of the uterine portion of the neck."

Churchill† describes its length as "being from 12 to 14 inches, its breadth from 9 to 10, and its depth, antero-posteriorly, from 8 to 9 inches."

Tyler Smith‡ says:—"At the end of gestation, the uterus is about 13 inches long by 8 or 9 in breadth,—its greatest antero-posterior diameter being 8 or 9 inches."

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* *Traité Complet de l'Art des Accouchements.* Bruxelles, 1835, p. 114.

† *Theory and Practice of Midwifery*, 1860, p. 92.

‡ *Manual of Obstetrics*, p. 184.

Dubois and Pajot* have the following:—"The uterus, in the state of vacuity, presents some slight variations in dimensions, and the measurements of it tend rather to rise above the average than to sink below it. During pregnancy, and especially at the full time, the uterus offers differences still more numerous; thus, whilst some observers have found uteri whose great diameter rose as high as 405 millimètres (16 inches), others have only found from 320 to 370 millimètres (13 to 15 inches), including the fundus, body, and neck." "Here are the averages which seem to us to approach nearest to the truth. For the vertical diameter, or the greatest length of the organ at the full time, 375 millimètres (15 inches); the transverse diameter in the broadest part 264 millimètres (10½ inches); and the antero-posterior diameter, at the greatest thickness, about 244 millimètres "(9⅝ inches). "Finally, the extent of the circumference of the organ, taken at the height of the tubes, is 70 to 73 centimètres (28 to 29 inches) on an average; and that of a smaller circumference, measured about 12 centimètres (5 inches) above the external orifice, is nearly from 34 to 38 centimètres" (14 to 15¼ inches).

Poppel† takes from Gassner the weight of the contents of the uterus as in round numbers 5 kilogrammes (10 lbs.), and as the specific gravity may be taken as nearly that of water, the volume will be 5 cubic decimètres (300 cubic inches). The diameter of a sphere of this volume is thence calculated as 8⅓ inches.

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* *Traité Complet de l'Art des Accouchements*, tome i. livr. 2me, p. 374.

† *Monatsschrift für Geburtskunde*, etc. 21 Bd. 1863, S. 8.

In calculations to be afterwards given, I assume the bulk of the uterus to be that of a prolate spheroid 12 inches long by 8 inches across. This assumption is made, to some extent, capriciously, or without exact grounds. It is done to enable other calculations to be made which are impossible in an unsymmetrical ovoid figure, but, to the mathematician, easy in the case of the figure assumed.

The superficial area of the uterus at the full time has been estimated by Levret at 339 square inches.

Poppel estimates it at 1358 quadratcentimètres, or 210 square inches, founding on the measurements above given.

Founding in like manner on the measurements of the prolate spheroid above given, the superficial area is found to be 250 square inches.

The formula for this calculation is,*

$$\frac{6{\cdot}283}{e}24\left\{\operatorname{Sin.}^{-1}e+e\sqrt{1-e^2}\right\}$$

where e, the eccentricity of the assumed elliptic section, is given by

$$e^2=\frac{6^2-4^2}{6^2}=\frac{5}{9}.\quad (\textit{Gregory's Examples}, \text{ p. } 438.)$$

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* I am indebted to Professor Tait for this and other contributions to the chapter.

The capacity of the gravid uterus at the full time is given by Levret as 408 cubic inches of water, which makes 17 lbs. in all, including the infant, the waters, the placenta, and its membranes.*

Poppel makes the capacity to be 5 cubic decimètres, or about 300 cubic inches, as already above described.

Supposing, as we have done, the volume of the gravid uterus at the full time to be equal that of a prolate spheroid of 12 by 8 inches, this gives the cubic contents as 400 cubic inches.

We have assumed that the surface of the gravid uterus measures 250 square inches. Now, as I have shown in another paper † that the effective pressure in ordinary labours per square inch varies from about 2½ lbs. to ¼ lb., it follows that the whole internal pressure on the uterus varies from 625 lbs. to 62½ lbs.; or, if the pressure rises to its highest, then it is about 1300 lbs.

Poppel's mean estimate of the same pressure is 313 kilogrammes (670 lbs.)

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* The difficulty of reconciling this with the weight of the contents is pointed out by Velpeau (*loc. cit.*) The confusion probably arises from variation or error in the estimated quantity of liquor amnii.

† *Trans. Roy. Soc.* of Edinburgh, vol. xxiv. 1867, and p. 304, etc. of this volume.

CHAPTER III.

ON CASES OF VAGINA DUPLEX ET UTERUS SIMPLEX AND OF SACCATED UTERUS.

I HAVE, in my own experience, met with several kinds of malformation of the female genital organs. Among such cases, none have been more remarkable than those of double vagina. I have seen three cases of vagina duplex. Of one of these, I had the misfortune to get only an imperfect examination : it was probably a case of *vagina duplex et uterus duplex ;* and therefore does not come into the same category with the other cases given in this chapter. But I may mention that in it the right vagina was of ordinary size, while the orifice of the left would not admit the little finger. The orifice of the left was like a very large urethral orifice, and its centre was considerably anterior to the centre of the other vaginal orifice, being nearly in a line with its anterior margin. A large sound, passed through the smaller orifice, could be easily felt through the left wall of the chief vagina, passing as high as the uterine cervix. This smaller vagina had no communication with the other. The genital organs were healthy.

The first case of *vagina duplex et uterus simplex* that came under my notice was in a lady who had come from a distance to be confined of her first child in Edinburgh. The nature of the case was recognised during labour, but not at first. There was a greater sensitiveness of one passage than of the other, a circumstance which, while the condition of parts was unknown, caused some anxiety; for then it was only a greater sensitiveness on some examinations than on others. But as the second stage progressed, chloroform was administered, and careful investigation of the parts instituted. The duplicity of the passage was then made out. But already it was impossible to say how high the dissepiment between the two passages was carried. For the advancing fœtal head pushed the dissepiment before it, apparently without lacerating it, as happened in the case recorded by Dr. Cappie. When the head presented at the vaginal orifice, it was checked by the dissepiment, now crumpled up from above downwards, and elongated from before obliquely backward and to the left side. Both vaginal orifices were distended, but the right was the largest.

Wishing to avoid laceration of the dissepiment, I cut it through, and the child was soon born thereafter. It was evident that the right vaginal orifice was used in cohabitation, while the left was not; and probably, on that account, it was more sensitive than the right, on a digital examination being made.

This case presented another rare anomaly. Before the child was born, and especially during a pain, a sort of hour-glass contraction could be distinctly felt. Above the ordinary outline of the large uterine body, and inclining towards the left hypochondrium, could be perceived a mass somewhat larger in size than a cocoa-nut, connected with the mass of the uterus by a considerable neck and not movable separately from that organ. This condition persisted until the separation of the placenta, when it could no longer be felt. It was therefore only in form an hour-glass contraction; in most other points of its history it differed from that well-known state of the uterus occurring after the birth of the child. This condition may be

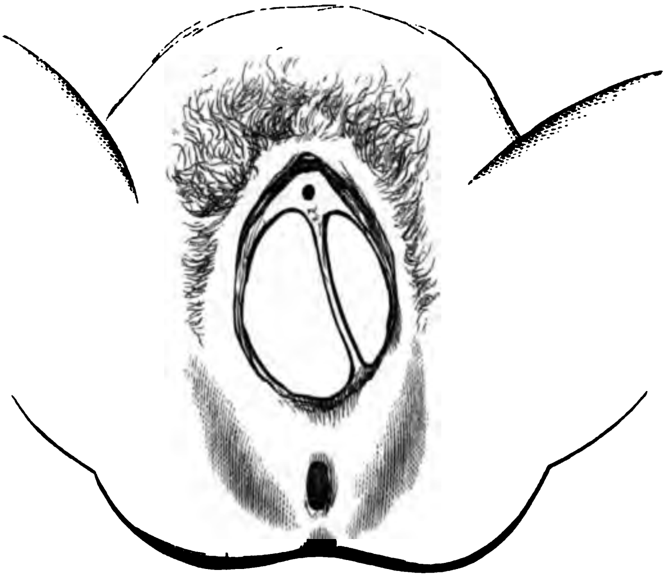

Fig. 38.

Fig. 38. Showing the vulva as distended by the advancing fœtal head.

called 'a saccated uterus'; and within the sac the placenta grew.

The condition of the placenta was curiously demonstrative of the saccated state of the uterus and of the site of its own insertion, for, only by assuming these two points as described, can the condition of the placenta be accounted for. Laying it on a board, it had the shape of a flattened globe. Its external or uterine surface touching the board, little else than external or uterine surface could be seen. From the upper surface of the spheroidal mass, as it lay on the

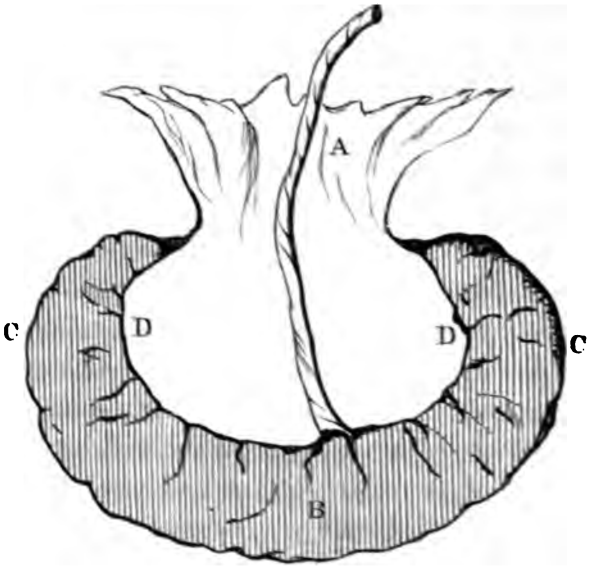

Fig. 39.

Fig. 39. Showing a plan of a section of the placenta. A, Lacerated membranes. B, Placental mass. C, External or uterine surface of placenta. D, Internal or membranous surface of placenta.

board, could be seen emerging membranes and cord, a considerable edge of the upper surface being the marginal part of the external or uterine surface of the placenta. It is evident that the placenta had grown within the sac, and that, during pregnancy, the membranes and cord passed through the neck of the sac to be continuous with the rest of the involucra and funis.

I shall not enter on any speculations as to the anatomy of this sac. The lady still lives, the mother now of several children not delivered under my superintendence, and any guessing as to the nature of the sac is vain, from want of such positive information as only an autopsy could afford. I shall only say that while the saccated condition may appear to some to indicate partial duplicity of the upper part of the uterus, it may by others be ranked with those curious cases of gestation, in which the placenta has been described as developed in a Fallopian tube while the fœtus grew in its normal site, or in which some other like anomaly has been represented as occurring.*

Peu, in the fifteenth chapter of the second book of his *Pratique des Accouchements*, published at Paris in 1694, describes a case of incarcerated placenta which he saw only after the birth of the child. His words show that he evidently thought that the placenta had been encysted during pregnancy or developed in a uterine sac; but there is nothing stated in the history of the case, or in regard to the anatomy of the placenta, which confirms his view; and the frequency of incarcerated placenta as compared with the extreme rarity of saccated uterus, renders it highly probable that the case was merely one of incarcerated placenta such as is not very rarely seen in cases of hour-glass contraction. Error of an opposite kind, yet somewhat analogous, was in vogue about the same time in regard to placenta prævia, the placenta being supposed to have fallen accidentally during labour into the situation in which it really grew. His words are as follows, and are both ingenious and important, whatever may have been the real nature of his medical colleague's wife's case.

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* For some remarks and references, see the *Edinburgh Medical Journal* for June 1856, p. 1059. See also Tanner, *Signs and Diseases of Pregnancy*, second edition, p. 274. For several other valuable references to Riecke, D'Outrepont, Aschern, Payan, and Scanzoni, see Grenser's *Lehrbuch der Geb.* S. 721.

"En l'année [illegible], Monsieur Martin l'ainé mon confrère, me fit l'honneur de m'apeller au secours de sa premiere femme, que je trouvai accouchée d'un puissant enfant, et non délivrée; car son délivre étoit resserré et enfermé du côté droit plus haut que le fond de sa matrice, comme si la nature lui en eût fait une seconde; sa sage-femme y trouva de la résistance, et lui résistant ellemême à son tour, rompit le cordon dans sa racine. Je ne la blâme pas d'avoir ignoré cette constitution de matrice peu ordinaire et inconnue à bien d'autres qu'elle; cette ignorance n'est pas un crime; mais de n'avoir pas demandé du secours dans une occasion où elle trouvoit un si grand obstacle. Je m'en plains, parcequ'en effet son impru-

dence non seulement jetta son accouchée dans la perte de sang, les hoquets continuels, les syncopes, et les sueurs froides, où je la trouvai : mais rendit aussi pour moi la démarche fort épineuse. Heureusement pourtant nous en sortîmes la malade et moi : elle revint en santé, et je fus depuis mandé en quelquesuns de ses travaux pour lui ménager la vie.

"La curiosité pourrait obliger quelqu'un à demander, comment un enfant peut subsister dans la matrice, aiant son délivre en un lieu séparé. Je répons là dessus, que c'est une de ces merveilles dont l'Auteur de la nature s'est réservé la connoissance. Je ne dirai point comment cela se passe. Peut-être le fétus et ce qui le suit est-il contenu d'abord et même engendré et formé dans ce lieu particulier dont nous parlons, et qu'ensuite le tout ne pouvant demeurer dans cet espace l'arriére-faix y reste, et le fétus décend sur les dernieres mois dans la matrice avec le tout, ou une partie des eaux ; sans cesser pour cela de recevoir ce qui lui est nécessaire, tant pour subsister que pour se perfectionner ; ni que rien empêche une libre communication entre le fétus, les eaux où il surnage, les membranes, qui contiennent ces eaux, et la masse où ces membranes sont appliquées. Quoi qu'il en soit ces sortes d'apotheques ou arriére-boutiques, m'ont toujours paru l'une des plus rares choses de ma profession ; et si je les ai trouvées garnis d'une espéce de marchandise de contre-bande qui m'a donné bien de la peine à faire passer : en recompense je me suis dédommagé sur le

plaisir que j'ai eu d'en faire la découverte à mon égard et d'y acquerir certaines lumiéres dont j'ai bien scu me servir ailleurs."[*]

For my second case of double vagina with single uterus, I am indebted to Dr. Warburton Begbie. The lady who was the subject of it had been recently married. She menstruated irregularly before marriage, the intervals between the periods being much exaggerated; but since her marriage the menses had recurred regularly with the usual interval. Her husband and she thought that there must be some unusual condition, because connection never seemed to be complete, and because attempt at connection was sometimes painful.

An examination at once discovered the state of matters. It was as follows:—There were two vaginal orifices; the right of ordinary size; the left only of capacity enough to transmit the finger easily. The right vagina was not unusually sensitive; the introduction of the finger into the left was at least very disagreeable to the patient. Each vaginal orifice had a well-developed frill-like hymen, a circumstance contradictory of the opinion of Hyrtl as to the absence of this membrane in such cases.[†] The septum between the vaginæ was thin, having, however, a thickening at its upper free end and at the part separating the lower orifices. The septum ended at the os uteri, the two vaginæ joining there. A finger could be passed through either vagina, bent over the upper edge of the septum, and pushed into the other vagina. The vaginæ were short, and, like the septum, measured only an inch and a half in length. When I first examined her she was pregnant. In the middle of her pregnancy I divided the septum with a wire ecraseur in the presence of Drs. Warburton Begbie and Russell, in order to avoid its laceration or artificial division during labour.

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[*] See some remarks on Peu's case by Baudelocque, *System of Midwifery*, Heath's Transl. vol. ii. p. 29.

[†] In Forensischer Hinsicht kann es wichtig sein zu wissen, dass bei angeborener Duplicität der Vagina der Hymen ohne ausnahme fehlt." *Handbuch der Topograph. Anat.* Von Joseph Hyrtl. *Zweite Auflage*, ii. Band, S. 103.

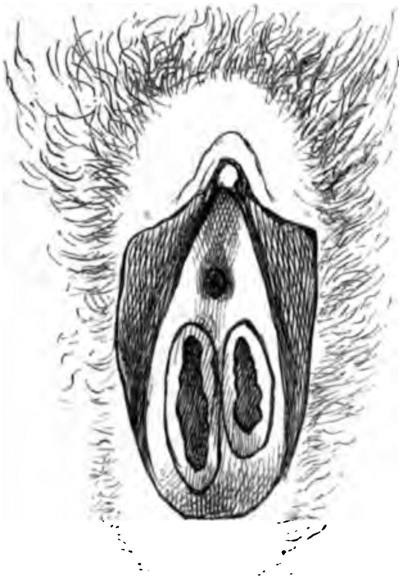

Fig. 40.

Fig. 40. Showing the vulva with the right and left vaginal orifices.

Labour was natural and easy. During it, the remains of the septum were easily felt in the form of a long crest on the anterior vaginal wall. As labour advanced, the lower part of this crest was protruded from the vulva, forming a considerable mass. During the delay of the child's head as it distended the perineum, this lower part of the remains of the septum increased in bulk and in hardness till it nearly equalled in dimensions the half of an erect penis.

Bands in the vagina, extending more or less directly from before backwards, have been described as partial dissepiments analogous in developmental origin and history to the septum of a double vagina. I have met with such bands, and observations of a like kind have been made by others.*

* Busch: *Geschlechtsleben des Weibes,* iii. Band, S. 159; Förster: *Die Missbildungen des Menschen.* Atlas, Tafel xx. Fig. 17; and by Dr. Alexander Simpson: *Edinburgh Medical Journal,* vol. ix. 1864, p. 957.

Kussmaul describes similar hymeneal bands (*Von dem Mangel, der Verkümmerung und Verdopplung, etc., der Gebärmutter,* S. 176,) and gives references to cases recorded or described by Moench, Senn, Mende, and Oldham. Klob, *Pathologische Anatomie der weiblichen Sexualorgane,* S. 417, also describes them, and cites a case of Campbell's. See also Kelch, *Beyträge zur Pathol. Anat.* S. 55; and Odier, *Comptes Rendus des Séances et Mémoires de la Soc. de. Biologie,* tome xviii. 1867, p. 73.

Many authors have, in their works, noticed the occurrence of cases of vagina duplex et uterus simplex, and have described some curious varieties of the condition, the chief of which are varying lengths of the dissepiment, closure of one of the vaginæ at its upper extremity or imperforation at the lower. Among these authors may be mentioned, Murat, *Dictionnaire des Sc. Méd.* tome lvi. p. 455; Rokitansky, *Pathological Anatomy,* Sydenham Transl. vol. ii. p. 265; Kiwisch, *Klinische Vorträge,* Abth. ii. S. 374; Farre, *Cyclopædia of Anatomy and Physiology,* vol. v. Suppl. p. 707 ; Courty, *Traité pratique des Maladies de l'Uterus et de ses Annexes*, p. 123.

The following references to cases of vagina duplex et uterus simplex are here given. I report them without pretending that the collection is complete, or placed in order. In the passages indicated, further references are given to the cases in connection with which only the observer's name is given by me.

Busch (*Geschlechtsleben des Weibes*, iii. Band, S. 159) mentions his having met with cases, and refers especially to a case by Stein and to another recorded in Rust's *Magazin*. Meissner (*Frauenzimmerkrankheiten*, i. Band, S. 346) notes a case by Callisen and another by Bartholin. Kussmaul (*Von dem Mangel, der Verkümmerung und Verdopplung der Gebärmutter, etc.* S. 176) cites cases recorded by Morgagni, Sixtus, Lemonier, Zentel, Carter, Guyot, Godard, and Dunglas. Förster (*Die Missbildungen des Menschen*, Atlas, Tafel xx. Fig. 16) gives a figure of a case. Maunoir (*Monatsschrift für Geburtskunde*, ii. Band, S. 503) records a case. A very interesting case, where the malformation occurred as a complication of labour, is narrated by Cappie, *Edinburgh Medical Journal*, 1864, vol. ix. p. 956. See also a collection of examples by Fürst for the cases observed by Hennig, Lisfranc, Spæth, and Strohl, *Monatsschr. F. Geb.* Sept. 1867, S. 204, etc.

APPENDIX.

REFERENCES TO PAPERS

APPENDIX.

REFERENCES to PAPERS, by the AUTHOR which have been, after more or less of change, introduced into this volume.

On the Statics of Pregnancy.—*Edinburgh Medical and Surgical Journal*, 1855.

This paper was the subject of a remarkable literary piracy by Dr. Helfft. See the *Edinburgh Medical Journal* for October 1855, p. 381.

On the Mode of Presentation of Dead Children in Labour.—*Association Medical Journal*, August 31, 1855.

On the Os Sacrum considered as forming part of the Vault of the Pelvis, and on its function in the Development of the Lateral Expansion of that Cavity.—*Edinburgh Medical Journal*, August and September 1855.

As the following letter may be interesting to some readers as contributing to the history of this subject, I insert it here:—

PARIS, *May 21st*, 1858.

To Dr. Matthews Duncan,
Lecturer on Midwifery in Edinburgh.

Sir,—I have just received from the hands of M. J. B. Baillière your interesting communication on the "Os Sacrum" (1855). I am indebted for this kindness, I suppose, to my own publication on the mechanism of the pelvis.

After having perused your very clear dissertation on the mechanical functions of the sacrum, I have seen, with the greatest satisfaction, we agree evidently in the point of view you have most neatly pointed out. Without the slightest doubt, the sacrum bone, mechanically, and as an intermediate between the vertebral column and the iliac bones, is exactly the reverse of a wedge.

Since some years, three or four about, your concept of the thing is mine. But I must confess, and I do willingly, the priority of the idea, according to the criterion of publicity, is doubtless yours. I wrote on the subject, for the first time, only in November 1856 (*Gazette Medicale de Paris*), and at this very time I was already anteceded by a Belgian professor, Dr. Hubert of Louvain, who, this same year, presented the Royal Academy of Belgium with a notice on the same theory.

But, as it is not probable Dr. Hubert has been more acquainted with your valuable paper on the subject than myself with his, when we have written on this point of mechanical anatomy, I am bound to believe, and you will too, I hope, that every one of us has been led to these conclusions by his own meditations.

Nevertheless, according to the dates of publicity, if the paper of Dr. Hubert I have alluded to is his first communication on the subject, the priority of this theory belongs certainly to you. If, as I wish, the opportunity is given to me, if the emission of a second edition of my *Traité de Mécanique Animale* allows me to do every one the right he is entitled to; be, sir, assured I will be eager in correcting in this way my former statement.

I speak only here of your physiological theory, which is the same for the three of us. As to your mechanical theory of the production of the pathological pelvis of Nægele, I do not agree so completely with Dr. Hubert or you. If you are so kind as to come back to my book (see the notes at the end of the volume), or to my articles of November 1856, the *Gazette Medicale de Paris*, you will note the differences existing, on this point, between us. You will find I consider the production of this sort of deformation of the normal pelvis as the consequence of a softening of one or of the two sacro-iliac articulations, in this last case unequally.

I am much indebted to you, sir, for your attention in sending to me your notice. I beg your indulgence on account of my poor and foreign English, with which I hope hardly to be understood.

Believe me, Sir, yours truly,
Dr. GIRAUD-TEULON.

56 Rue de Provence, Paris.

Notes on the Formation of the Ricketty and Malacosteon Pelves chiefly after the researches of M. Meyer of Zurich.—*Edinburgh Medical Journal*, April 1856.

On the Development of the Female Pelvis.—*Edinburgh Medical Journal*, October and December 1859.

The Behaviour of the Pelvic Articulations in the Mechanism of Parturition.—*Dublin Quarterly Journal of Medical Science*, August 1854.

The Theory of Menstruation in early Pregnancy, Superfœtation, and the Site of Insertion of the Ovum.—*Monthly Journal of Medical Science*, April 1853.

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THE END.

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FECUNDITY, FERTILITY, STERILITY

AND ALLIED TOPICS.

BY J. MATTHEWS DUNCAN, M.D.,

Lecturer on Midwifery in Surgeons' Hall Medical School,
Physician for Diseases of Women to the Royal Infirmary, Edinburgh,
etc. etc.

CONTENTS.

(1) Population at Different Ages. (2) On Weight of Newly-born Child. (3) Production of Twins. (4) Fertility of Marriages. (5) Families of different Numbers. (6) Fertility and Fecundity. (7) Mortality of Childbed. (8) The Age of Nubility. (9) Duration of Labour. (10) Insemination and Conception, etc. etc.

CRITICAL NOTICES.

From the Lancet.

We owe many apologies to Dr. Duncan for having so long delayed a notice of his most valuable and important work. The truth is, it contains so much matter needing calm and attentive study, that we have been waiting for leisure to read, mark, learn, and inwardly digest the three hundred and sixty pages devoted to the analysis of facts from which deductions of a very remarkable and interesting character, in reference to the laws affecting the reproduction of the human species, have been arrived at. In truth, every one of the numerous chapters into which the author has classified his materials well merits distinctive critical analysis, and we are not without hope of so dealing with certain of them as opportunities may arise.

That with all the elaborate machinery of registration in operation for thirty years in England and for twelve years in Scotland, data for determining such a moot point as the relative power of reproduction among women at different ages, and under other conditions variable within ascertainable limits, should be entirely wanting for England and only procurable for one year in Scotland, is not a little astonishing; but it is, nevertheless, a fact recorded by Dr. Duncan. The English birth-schedules contained nothing to throw light on some of the particulars needful to be ascertained; but when the systematic registration of births was established in Scotland, the schedule in use exacted from the public a variety of interesting details—a circumstance which gives to the registers for 1865 (the only complete year of their use prior to the adoption of a less comprehensive schedule) an extraordinary value. Dr. Duncan had further to confine his inquiry within limits compatible with the exigencies of time, labour, and expense; and hence his selection of the registers of Edinburgh and Glasgow, which yielded a collective observation of 16,593 children, all *legitimately* born in 1855. A year's birth-statistics of these two cities, then, constitute the foundation upon which, with a painstaking and acute analysis, has been erected a superstructure of logical inference of the most practical kind, calculated especially to direct attention to matters that have hitherto remained in comparative obscurity.

Part I. of Dr. Duncan's investigation relates to the determination of the comparative fertility or productiveness and fecundity of women at different ages; and in order to avoid confusion he defines fertility or productiveness to mean "the amount of births as distinguished from the capability to bear;" fecundity, meaning the demonstrated capability to bear children, "implies the conditions necessary for conception in the women of whom its variations are predicated. . . In short, fertility implies fecundity, and also introduces the idea of number of progeny; while fecundity simply indicates the quality without any superadded notion of quantity." The general conclusions under this first head are—1. That the great majority of the population is recruited from women under thirty years of age; but that the mass of women in the population between thirty and forty contribute a larger proportional share to the general fertility than do the women between twenty and thirty. 2. That the wives in the population taken collectively show a gradually decreasing fecundity as age advances; but that in individual wives the degree of fecundity increases till about the age of twenty-five, and then diminishes. The individual fecundity is described as forming a wave which, from sterility, rises gradually to its highest, and then more gradually subsides again to sterility.

In Part II. the author treats of the weight and length of the newly-born child as indicative of the state of fecundity, or of the generative functional vigour, of the mother; the data in this instance being drawn from the records of 2070 pregnancies, with 2087 children, in the Edinburgh Royal Maternity Hospital. Dr. Duncan's view is that increase of weight and length of the child is in direct dependence on the age of the mother; and that a careful study of the subject goes to support the doctrine that the vigour of the female reproductive system waxes till about the age of twenty-five, and then wanes. Professor Hecker's researches confirm the influence of age, but they indicate an additional element in the number of the pregnancy.

Part III. is devoted to the elucidation of some laws relative to the production of twins, as to which the following are among the conclusions arrived at:—1. The largest number of twins is produced by women between the ages of twenty-five and twenty-nine. 2. The mean age of twin-bearing mothers is greater than that of mothers generally. 3. Newly-married women are more likely to have twins the older they are. 4. A woman is more likely to have twins in each succeeding pregnancy than in the former pregnancy; the first pregnancy, however, forming an exception. 5. It is probable that twin-bearing women have larger families than women uniformly uniparous. It is stated that among women the birth of twins occurs once in about eighty deliveries.

In the numerous sections of Parts IV., V., and VI. are discussed the laws of the fertility of marriage; it is designated, "sustained fecundity," or the fertility of women cohabiting with men during the child-bearing period of life,—and also those of sterility; the mathematical skill of Professor Tait having been enlisted for the expression by certain curves and formulæ of the laws demonstrated by Dr. Duncan. A little rubbing up of one's knowledge of the signs and symbols of algebraic notation is essential to an appreciation of Professor Tait's mode of arriving at the general law that "fecundity is proportional to the number of years a woman's age is under fifty;" and it would occupy much more space than we can now afford were we to attempt even a partial summary of the hundred pages wherein Dr. Duncan has gathered together a mass of statistics illustrative of the fertility of marriage under a multiplicity of circumstances as to age, etc., the comparative fecundity and fertility of different peoples, and the probabilities of sterile marriage. Under this latter head we may, however, particularise one or two laws possessing a certain interest. 1. That the question of a woman being probably sterile is decided in three years of married life. 2. The older a fertile woman is at marriage, the older is she before her fertility is exhausted—that is, before the advent of relative sterility. 3. A wife who, having had children, has ceased for three years to exhibit fertility, has probably become relatively sterile—that is, will probably bear no more children, the probability increasing as time elapses. Dr. Duncan says that these conclusions will help medical men to estimate "the utility of the many vaunted methods of curing sterility which are now much in vogue, and which, considering the nature of the condition to be cured, justly excite anxiety for the honour of the profession in the minds of its best friends."

We come now to the two important questions in reference to puerperal mortality discussed in Part VII. Does the number of a woman's pregnancy regulate in any degree the mortality to be expected from lying-in? Does the age of the child-bearing woman regulate in any degree the mortality accompanying this function? To the first of these questions it is answered that the mortality of first labours is about twice as great as in all subsequent labours put together, the fatality of puerperal fever being in the same proportion; and that after the ninth labour the risk of death increases with the number. The age of least mortality is near twenty-five years, and from that point it gradually increases with the diminution or increase of age, the age of greatest safety in parturition coinciding with the age of greatest fecundity. Assuming the correctness of these inferences, it is clear that, as Dr. Duncan observes, a comparison of the mortalities of lying-in institutions cannot justly be made unless the conditions of primiparity and age be taken into account.

In Part VIII. the author points out the ages within which women generally should enter the married state, if they are guided by physiological laws; and it is shown that as the period between twenty and twenty-five years is that in which marriage is found to be most secure of fecundity, and parturition attended with least danger, that is the best time for women to get married. This has reference to the safety of the mother; but it is found, also, that there is a greater survival of children born of women married between twenty and twenty-five than at any other ages, and thus there is another reason for the period selected.

Parts IX. and X. are devoted to the following propositions in reference to the duration of labour and pregnancy: 1. The mortality of women in parturition and childbed increases with the duration of labour. 2. The duration of labour is only an inconsiderable item among the many causes of the mortality of women in parturition and childbed. 3. That the real duration of pregnancy (the interval between conception and parturition) has not been exactly ascertained in any case. 4. That the average interval between insemination and parturition (commonly called the duration of pregnancy) is 275 days. 5. That the average interval between the end of menstruation and parturition is 278 days. 6. That neither of the intervals just referred to has a standard length, but varies within certain limits. 7. That there is evidence to establish the probability that real pregnancy may be protracted beyond its usual limits to the extent of three or four weeks, or even longer.

We have thus endeavoured to convey to our readers a general impression of the characteristics of certainly one of the most interesting contributions to medical statistics which we have ever perused. We are not prepared at the present time to discuss critically many points as to which difference of opinion will arise. Statistics, as we all know, have a name for being convertible according to the fancy of the manipulator; and it might possibly appear on close examination that some of the data used by Dr. Duncan are rather more limited than we should consider safe for formulating laws on the abstruse and complicated functions of reproduction. We say this, however, not with the least intention of depreciating the value of Dr. Duncan's investigations. The want of sufficient data was the greatest difficulty he had to contend with, and the marvel is that he has been able so fully to establish as much as he has done. Not one of the subjects treated but has a peculiar interest for the medical profession; and we therefore very earnestly recommend the study of the book to our readers.

From the Edinburgh Medical Journal.

BOTH from the great labour, care, and skill, expended in the working out of details, and from the importance of the results, as either new or confirmatory of what was previously known or merely conjectured, the work is one of sterling value. It forms an original and important contribution, not only to obstetric science, but also to the department of political economy which treats of population, and to the principles of life insurance. Within our limits it is not possible to enter fully into all the topics discussed. No justice can be done to the inquiries without a study of the book itself. Within the limits assigned to us we have been able to give only a very inadequate idea of the rich of fact and inference which this volume contains, yet enough has said to justify and explain our recommendation of it as a work re with original and valuable information, the study of which is i pensable to all who are interested in the subjects of which it trea who are engaged in similar inquiries.

From the Medical Times and Gazette.

FREELY as we have extracted from the stores contained in this vol we can yet assure our readers that the mine is far from exhausted : would strongly urge them to read it carefully for themselves. To Duncan, who is well known as one of the most distinguished Sco physicians of the present day, we tender, in the name of this jou the thanks of the profession for having presented us with a stan work, in which the results of former inquiries in the same departm of knowledge are judiciously blended with a large mass of orig matter.

From the Scotsman.

THE questions treated of in the volume before us not only affect health and happiness of individuals and families, but exercise important influence on the prosperity of states. Dr. Duncan has ducted his researches in a most complete and comprehensive man and has furnished us with a contribution to vital statistics valuable, both from its elucidation of ascertained facts and from gaps and uncertainties in our knowledge which it has shown to e It supplies us at once with an epitome of all that is known on subject of which it treats, and places us on a firm basis from whic advance to new acquisitions.

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